PROCEEDINGS
OF THE
AMERICAN ACADEMY
OF
ARTS AND SCIENCES

VOL. LXXX

CONTRIBUTIONS TO THE ANALYSIS AND SYNTHESIS OF KNOWLEDGE

Published in cooperation with the
INSTITUTE FOR THE UNITY OF SCIENCE

BOSTON:
PUBLISHED BY THE ACADEMY
1951 – 1954

Proceedings of the American Academy of Arts and Sciences

VOLUME 80

CONTENTS

ISSUE PAGE

1. *Introductory Remarks*, PHILIPP FRANK 5
Greetings, JAMES B. CONANT 9
The Logical and Sociological Aspects of Science, PHILIPP FRANK 16
Integration in the Biological and Social Sciences, W. H. SHELDON 31
The Science of Man and Unified Science, CHARLES MORRIS 37
The Verifiability Theory of Meaning, HANS REICHENBACH 46
The Concept of Cognitive Significance: A Reconsideration, CARL G. HEMPEL 61
Comments on Professor Hempel's Paper, GUSTAV BERGMANN 78
Natural and Scientific Language, JOHN LOTZ 87
Semantics and Abstract Objects, W. V. QUINE 90
Comments on the Paper of Professor Quine, MAX BLACK . . 97
The Need for Abstract Entities in Semantic Analysis, ALONZO CHURCH 100

2. *The Origin of the Separation between Science and Philosophy*, PHILIPP FRANK 115
Brief Bibliography for the Sociology of Science, BERNARD BARBER and ROBERT K. MERTON 140
Brief Bibliography of Formal Logic, ALONZO CHURCH . . 155
Some Significant Trends toward Integration in the Sciences of Man, LAURA THOMPSON 173

3. *Inductive Logic and Science*, R. CARNAP 189
On Mental Entities, W. V. QUINE 198
A Bibliography of Cybernetics, F. D. BARRETT and H. A. SHEPARD 204
Significs, Its Tendency, Methodology and Applications, D. VUYSJE 223

4. *Psychoanalysis and the Unity of Science*, ELSE FRENKEL-BRUNSWIK 271
Index to Volume LXXX 349

Proceedings of the American Academy of Arts and Sciences

VOL. 80, NO. 1, P. 1-112, JULY, 1951

CONTRIBUTIONS TO THE ANALYSIS AND SYNTHESIS OF KNOWLEDGE

Published in cooperation with the
INSTITUTE FOR THE UNITY OF SCIENCE

In accordance with the policy of the American Academy of Arts and Sciences to encourage publication of papers transcending the boundaries of separate disciplines, this issue of the Proceedings represents the first of a series of Contributions to the Analysis and Synthesis of Knowledge which the Academy expects to publish in cooperation with the Institute for the Unity of Science. This and subsequent numbers comprising the prospective series may be obtained independently of other issues of the Proceedings at the price designated on the inside of the cover by addressing the American Academy of Arts and Sciences, 28 Newbury Street, Boston 16, Massachusetts.

For the Committee on Publication

ERNEST H. HUNTRESS,
Acting Editor

CONTENTS

Introductory Remarks, PHILIPP FRANK 5

Greetings, JAMES B. CONANT 9

Part 1 *Science and Man*

The Logical and Sociological Aspects of Science, PHILIPP FRANK 16

Integration in the Biological and Social Sciences, W. H. SHELDON 31

The Science of Man and Unified Science, CHARLES MORRIS 37

Part 2 *The "Meaning" of Scientific Statements*

The Verifiability Theory of Meaning, HANS REICHENBACH 46

The Concept of Cognitive Significance: A Reconsideration, CARL G. HEMPEL 61

Comments on Professor Hempel's Paper, GUSTAV BERGMANN 78

Natural and Scientific Language, JOHN LOTZ 87

Part 3 *The Role of "Abstract Objects" in Science*

Semantics and Abstract Objects, W. V. QUINE 90

Comments on the Paper of Professor Quine, MAX BLACK . 97

The Need for Abstract Entities in Semantic Analysis, ALONZO CHURCH 100

INTRODUCTORY REMARKS

In his inaugural address on "The Future of the Academy", delivered in October, 1944, President Howard Mumford Jones pointed out that, while "all the forces leading to the fractation of knowledge have been more vigorously operative in the last forty years than ever before", there are two forces for unity.

First, "in some sense specialism has defeated itself. That is to say, what once looked like a separate compartment of knowledge, unique and apart from everything else, the more it is explored, the more its boundary lines seem to waver and vanish. The relation of fields is not that of pigeon-holes, but of living, interdependent cells. The historian depends upon the anthropologist, the anthropologist upon the sociologist, the sociologist upon the psychologist, the psychologist upon the biologist, the biologist upon the chemist, the chemist upon the physicist, the physicist upon the mathematician, and so on in an endless circle. . . . The very multiplication of specialties has forced the specialists to come together, to exchange ideas, to study each other's techniques, even—what is anathema to the old-fashioned, hard-boiled, realistic investigator—to discuss the philosophy of what they are doing."

"And a second pressure towards unity has come from the outside. Students of Western culture have freely said that if we do not recover a common set of ideas, a common tradition, and a common language, we are lost."

He then suggested that an Academy embracing all fields of knowledge, such as the American Academy of Arts and Sciences, was a particularly suitable instrument for intercommunication across the boundaries of the various disciplines, and asked whether it was not in fact the duty of this Academy to work toward the creation of "a language common to all disciplines".

Following these suggestions of President Jones, the Academy appointed a committee (under the chairmanship of Kirtley Mather) to develop concrete recommendations. The report of this Commission on the Present Status and Future of the Academy (October, 1946) indicated that "tacit in the smoothly rounded phrases of the Charter of Incorporation and other early documents is the thought that the spirit, purpose and essential logical and instrumental methodology of science can be applied more or less readily and successfully to any and every form and aspect of knowledge.'' Moreover, the fact that the Academy ''embraces all the scholarly disciplines . . . provides a basis for *cross-fertilization among the various fields of intellectual endeavor* and an impetus for synthesis of ideas and information, much needed to offset the extreme departmentalization of modern thought.'' The report said that ''increasing specialization of knowledge is tending more and more to produce . . . 'uneducated experts' '' and called for ''humanization of knowledge'' which does not mean an emphasis on humanities in contrast to the sciences, but an integration of the sciences with the humanities. ''It transcends by far the idea of synthesis of knowledge as contrasted with the analysis of nature.''

The ''Institute for the Unity of Science'' was founded in the year 1947. Its purpose was defined in its charter as ''to encourage the integration of knowledge by scientific methods, to conduct research in the psychological and sociological backgrounds of science.'' The Institute represented a group of scientists and scholars (many of them members of the Academy) who felt a need similar to that so eloquently expressed by President Jones and by the report of the Mather Committee. The Institute is cooperating with the Academy in publishing a series of the Proceedings devoted to contributions to the ''Synthesis and Analysis of Knowledge.'' By this title we wanted to describe in an abbreviated way two tasks which have been frequently regarded as contradicting each other. One has contrasted the logical analysis with the speculative synthesis of knowledge. According to our view, these two tasks complement one another and are fundamentally one and the same. By analyzing knowledge, we find the elementary operations of which our scientific knowledge consists. But we recognize that these elementary operations are essentially the same ones in all fields of science and learning. They can serve as the building stones for the synthesis and integration of our knowledge in all fields. This means the construction of a picture of the world which is speculative, in the sense that atomic physics is speculative. But it is analytical in the sense that the sciences of mechanics or genetics or economics are analytical. The field of research that we have described as ''analysis and synthesis of knowledge'' has been frequently called the ''philosophy of science.'' As we work for an integration between the sciences and the humanities, we soon recognize the focal importance of the integration between science and philosophy. At this point, the departmentalization in our system of research and education has been carried to a self-destructive stage: "philosophy", which in keeping with its tradition should take the leadership in the fight for integration, has become itself an isolated "department." We produce "doctors of philosophy" who are either ignorant in philosophy or ignorant in science.

One of the essential means of combating the prevalence of what President Jones called "uneducated experts" is to free philosophy from its isolated role and to restore it to its place as a link between the arts and sciences with the active cooperation of the workers in both areas. When August Comte in his "Positive Philosophy" attempted to establish a philosophy which should be essentially an instrument of integration, he claimed, not in vain, that he intended to restore to philosophy the place allotted to it by Aristotle.

This first issue of the new series of the PROCEEDINGS contains papers presented at the first national conference held under the sponsorship of the Institute for the Unity of Science at the American Academy of Arts and Sciences in April, 1950.

The plan of this meeting was to discuss some issues which have been focal points in the approaches towards an integration of knowledge. During recent decades, substantial progress has been achieved by considering the sciences as formal systems and by analyzing them from the logical and semantical viewpoint—this means from the viewpoint of their internal coherence and from the viewpoint of their meaning in the world of our experience. This kind of analysis is the topic of Parts II and III of the conference.

However, it has turned out more and more that these problems cannot be settled definitely on the basis of logical and semantical analysis. There remain always several possibilities for the choice of a formal system. Carnap contrasted recently in an excellent way the "internal" problems which can be solved by logic and semantics with the "external" problems. The latter ones put the question whether a certain formal system, as a whole, with the addition of a semantical interpretation, is useful for the orientation of man in the world of experience. Here we turn from the logical and semantical to the pragmatical viewpoint. Carnap stressed, for example, the point that the problem of whether one has to introduce "abstract objects" depends in the last analysis upon for what purpose one introduces them. The same resort to pragmatic criteria we find in Hempel's treatment of the problem of "meaning." In the last year, Quine has even advanced the opinion that also the distinction between analytical and synthetical propositions depends ultimately on pragmatic criteria.

However, if we want to speak in terms of actual science, what kind of argument do we call "pragmatic"? To get the answer we have to consider science as a human enterprise by which man tries to adapt himself to the external world. Then a "pragmatic" criterion means, exactly speaking, the introduction of psychological and sociological considerations into every science, even into physics and chemistry.

It seems, therefore, that the sociology of science, the consideration of science as a human enterprise, has to be connected in a very tight way with every consideration which one may call logical or semantical. But, of course, we can only reach this cooperation by building up a sociological theory of science in addition to the logical one, and by working out both approaches until they will meet each other. For these reasons, the papers on "Science and Man" form the first part of this issue.

It is widely felt in the Institute for the Unity of Science that by the combination of the logical and the sociological approach to science all the needs which have produced traditional philosophy and metaphysics can be satisfied.

PHILIPP FRANK

GREETINGS TO THE NATIONAL CONFERENCE OF THE INSTITUTE FOR THE UNITY OF SCIENCE
BOSTON, MASSACHUSETTS — APRIL, 1950

J. B. Conant
President, Harvard University

Professor Frank, Ladies and Gentlemen:

I appreciate the honor of being asked to say a few words at the opening of this conference. For I feel sure that what you are endeavoring to accomplish in the formation of an Institute for the Unity of Science is of great importance. Both as a college president and a chemist I have personal reasons for being deeply interested in the unity of science. As Professor Frank has indicated in his kind introduction, I have recently experimented with the presentation of science to undergraduates as part of a general education. If we are going to give the lawyer, the statesman, the business man, the newswriter some understanding of science, we educators must ourselves endeavor to understand the methods of science.

As a university president I have had the privilege of sitting as chairman of *ad hoc* committees concerned with appointments to professorships in a great variety of fields of learning. Such an unusual educational experience enables me, however superficially, to know something of the accomplishments and ambitions of the scholars of today. The unity I see among all the learned men of our universities is not a unity of method, but a common agreement as to certain premises that almost unconsciously guide their actions. It is assumed that new knowledge is of the utmost importance, that a man's value to a community of scholars rests not on what he knows but on what new ideas he may contribute. I suggest that since the Renaissance, at least, it has been the dynamic nature of the undertaking that has provided the unity between natural scientists, grammarians, historians and philosophers.

I have spoken of new knowledge and new ideas. I take it one of the chief problems facing the whole learned world today is how to evaluate intellectual labor—one's own program as well as those of one's colleagues. Stamp collecting, as far as I know, has never been admitted into the academic circle. There are no chairs dealing with this subject, yet many men have labored long and catalogued a vast amount of information about the symbols used in a highly important activity of modern man. Why not stamp collecting? I suggest this question might be considered by scientists and scholars from time to time. And lest this question seem to point a possible finger of scorn at those professors who turn over endless dusty pages of manuscripts or rare books let me address a question to my fellow chemists. Why not a professorship of the culinary art? Why has not cooking been added to the branches of applied chemistry taught in our universities?

Of course some of you will be quick to reply to these questions that some activities of scholars and scientists are little better than stamp collecting or cooking and should be purged from a true university. Perhaps your criteria will be in terms of definitions of natural science as a quest for the ultimate structure of the physical universe, of the humanities and social sciences as endeavors to understand the nature and destiny of man. I should draw the line between "proper" and "improper" academic subjects on a different basis, for my own definition of science as part of accumulative knowledge is not in terms of any final goal but the nature of the process. The essence of the undertaking of the historian, the social scientist, the philosopher, the natural scientist to my mind lies in the fact that his efforts will be measured by their fruitfulness, not by their finality. Even physical science, to my mind, is not a quest for certainty but rather a quest which is successful only to the degree that it is continuous. For science according to my view is best defined as "a series of interconnected concepts and conceptual schemes, schemes arising from experiment and observation and fruitful of new experiments and observations."

Science thus defined, I hardly need remind you, can be identified with the new "experimental philosophy" of the 17th century. The study of scholars of the last few decades has shown, however, that in the rapid growth of mechanics in the 17th century, the point of view of that century was not as new as historians once thought. Rather the type of reasoning that may be called deductive or geometrical was of prime importance and the history of this type of intellectual activity can be traced back well into the Middle Ages; it is, of course, well exemplified by the writings of Archimedes. The union of this type of scholarly activity with the cut-and-try type of experimentation long practiced by the practical men (craftsmen, agriculturists, artisans) produced modern science.

The union required many generations for completion and perhaps not before the late 18th century can we see the methods of experimental science in full swing. The method of "geometrical reasoning" is illustrated not only by the writings of Archimedes, but also by those of Stevin of Bruges and Pascal on hydrostatics. As to the practical arts, any number of examples might be chosen. The winning of metals from their ores is highly instructive. It is quite clear that the rules and recipes of the smelters of the 16th, 17th, and 18th centuries were highly satisfactory in terms of practical results. The men of those centuries were in command of a vast amount of organized (though hardly systematized) knowledge; they were able to identify rocks, "earths," in terms of what they would yield when smelted. All this information and the rules for procedure are quite similar to the recipes in a modern cookbook. Relatively few concepts are to be found in such books other than those we designate as "common-sense ideas" (e.g., spoons, the process of mixing, baking, etc., "salt," flour, etc.). For the metal makers of the 16th century the Aristotelian principles as modified by the alchemists provided some general points of reference ("sulfur," "earth," "fire," "water"). With the introduction of the phlogiston theory a first attempt was made at a working hypothesis on a grand scale which was relevant to the work of the "sooty empiric." From this broad hypothesis which was soon so widely adopted as to become a conceptual scheme, a whole host of deductions followed which could be connected with experimental findings.

The phlogiston theory reduced the degree of empiricism somewhat in practical chemistry (smelting) and was fruitful of much significant chemical work. However, practical men made relatively little use of the phlogiston theory; indeed, the practical books on metal making of the 18th century barely refer to any theory. With the advent of the new chemistry of Lavoisier and the atomic theory of Dalton, mineralogy and metallurgy begin to become "scientific." The degree of empiricism in metal making and in mineralogy dropped rapidly. But in spite of the developments of physics and chemistry from 1800 to 1950, metallurgy is still an art with a considerable degree of empiricism.

The distinction between scientific advance and empirical innovation is to be sought then in terms of what is usually called "theory." In reviewing the early phases of the history of each science the distinction is relatively clear-cut. But in this century so many concepts of science and so much of the conceptual scheme has become embedded in common sense that the distinction is one of degree and not of kind. The definition of science as "attempts to lower the degree of empiricism in a given area" thus centers attention on those aspects of scientific inquiry that most clearly distinguish such activities from the empirical studies of the inventor. It likewise defines the boundary between science and such purely empirical endeavors as cooking and stamp collecting.

Of course, no investigator in his right mind prefers empiricism to theory. Everyone would like to have clear-cut operational definitions of all the variables, but when this is accomplished, the problem is nearly solved! Therefore, there is no use berating one group of scientists for their failure to use concepts that are clearly related to experiments or observations; it is only helpful if one can suggest specifically how the "fuzzy" nature of many scientific ideas can be made less "fuzzy." In the history of every branch of science the early investigators started with the current common-sense notions which were bound to be hazy criteria for identifying the variables and procedures. Often the best that even a genius can do is to try to relate one set of common-sense ideas to another set. He comes pretty close to arguing in a circle as did Pasteur in his study of fermentation. Very close to a circle, but not quite; rather a spiral argumentation, we may say! Vicious as this reasoning is from the point of view of logicians, it has proved in practice more than once to have been the mode of activity which opened up new fields. I venture to think that if Pasteur, for example, had been more deeply imbued with the "principles of right thinking," he would never have advanced biochemistry as he did. In other words, if one waited for clear-cut operational definitions and were unwilling to argue *almost* in a circle, progress in some areas might never be made at all. If this be true, it is of importance for the methods of science and significant for the application of the methods of science to fields now entirely empirical.

If one can judge the future by the past, the further reduction of empiricism in the biological sciences (including psychology) and the social sciences will come as men of genius have ideas of a general nature which can be related to observations and experiments, or as new instruments or techniques make available data which again men of genius can successfully use as a basis for new or enlarged concepts. In the meantime one must be content to use as intelligently as one can the empirical procedures which are available. This would seem to apply to organic chemistry, experimental biology, psychology, and the social sciences.

If this be true, exhortations to people to be "scientific" or to substitute "controlled inquiry" for judgments based on mere opinion would seem to be somewhat beside the mark. A farmer of intelligence engaged in buying or selling a horse is likely to be as free from prejudice or emotions as a scientist in his laboratory. A shrewd investor looking into the potentialities of a new business, a lawyer giving an opinion to his client as to the possibility of a court action, are all engaged in trying to form an impartial judgment. On the other hand, a scientist or engineer arguing for the construction of a pilot plant to demonstrate his invention may be carried away by that emotion often labeled as the pride of authorship. So too may be an educator who is arguing for his own pedagogic devices be they a definition of science or a set of words to relate science and invention.

With this last thought in mind, I realize it is time for me to conclude. I do so by once again congratulating the members of this conference. Whether you agree or not with my emphasis on the dynamic nature of science, I am sure you will agree that a quest for unity in science is a forward-moving enterprise. As one way of attacking the intellectual problems of this troubled century, your Institute is certain to be of service. All the members of the learned world must wish you well in this endeavor whether they agree with your basic philosophy or not. That your combined efforts may be fruitful of ideas yet unthought of is my own hope, as I bring you greetings here today.

PART 1

SCIENCE AND MAN

THE LOGICAL AND SOCIOLOGICAL ASPECTS OF SCIENCE

Philipp Frank
Harvard University

1. Plato's Views on "Science and Government"

In his book, *The Laws,* Plato discusses the attitude which an ideal government should take towards science. Briefly speaking, he regards science not so much as a search for truth as an instrument for the education of "good citizens". He deplores, like many educators and politicians of our own age, the bad influence of science on religious and political education.

In *The Laws,* the "Athenian Stranger" who advocates Plato's own views says:

> The theories of our modern men of enlightenment must be held to account for the mischief they cause. Now the effect of their compositions is this: when you and I produce our evidence of the existence of gods and allege this very point — the deity or divinity of sun and moon, planets and earth — the converts of these sages will reply that they are but earth and stones, incapable of minding human conduct, however plausibly we have coated them over with a varnish of sugared eloquence.

The Cretan, Clinias, replies to this, "A dreadful theory that you are talking of, sir, even if there were only one such! How much more dreadful our present age were such doctrines so abundant".

The doctrine which Plato regards as dreadful is simply commonplace today. In every elementary course in physics, chemistry or astronomy, we learn that the sun and planets are made of the same chemical elements we find on earth. However, Plato is concerned lest this dangerous doctrine of astrophysics lead to impiety, and the "Athenian Stranger" suggests measures to prohibit the teaching of such doctrines. He says:

> For the disobedient, a law against impiety may run as follows: if any man commits impiety of word or action, any person present shall defend the law by giving information to the magistrate. The first magistrate under whose notice the matter comes shall bring the

> case before the court appointed to deal with such offenses as the law directs.

In order to remove the danger that some official may be lenient towards such bad doctrines, the "Athenian Stranger" makes the following suggestion:

> Any official failing to take action on information received shall himself be liable to be proceeded against for impiety at the suit of anyone willing to vindicate the law. In the case of conviction, the court shall impose a particular penalty on the offender for each act of impiety. Imprisonment shall form part of the penalty in all cases.

We note here how much the attitude of the "ideal government" towards science differs from the attitude which a modern scientist would regard as "ideal". For the scientist, the first requirement is that the principles of science should be "true". This means, for the scientist, that these principles should be confirmed by sense-observations and logical conclusions. But the authorities who are responsible for the good conduct of citizens require, in addition to "scientific truth", that the principles of science should be useful, or at least not harmful, in the education of "good citizens". These authorities may be the state, the church, the school, or the opinion of the "man in the street". Their chief interest is not in the "truth" of a scientific doctrine but in its '"goodness", as we may call it, meaning its usefulness for the education of "good citizens".

There is an inherent difference between these requirements for "truth" and for "goodness" in scientific doctrines. Those who make the first requirement judge a doctrine according to the "logico-empirical" criteria of science; those who make the second requirement judge a doctrine according to its "effect on the social behavior of citizens". One can, therefore, speak of judging a scientific doctrine from the "logico-empirical" *or* from the "sociological" aspect. If we mean by "logico-empirical" aspect the checking of a scientific doctrine by experiments and logical deductions, there is no doubt that Plato stressed the "sociological" aspect. We recognize this very clearly if we look into the seventh book of his *Republic* where he outlines a curriculum for the training of leaders in his ideal state. He speaks about the study of arithmetic, geometry and astronomy. One sees very clearly that he suggests teaching these subjects not as systematizations of man's sense experience nor as methods to govern the physical world. He sees arithmetic, geometry and astronomy as paths by which the pupil can march towards the ideas of perfection, beauty and harmony. These ideas in turn will lead him towards the idea of the "good". And to be imbued with this idea is, according to Plato, the ultimate goal in the education of a "good citizen".

Plato proposes that the leaders in his ideal state must understand, for example, the nature of numbers not "like merchants or retail traders, with a view to buying or selling, but for the sake of their military use, and of the soul itself". The leader, as Plato conceives him, should be "soldier and philosopher in one". Speaking of geometry, Plato acknowledges its usefulness for the military art:

> An officer who had studied geometry would be a very different person from what he would be if he had not . . . But for such purposes a slight modicum of geometry and calculation would suffice. What we have to consider is whether the greater and more advanced part of it tends to facilitate the appreciation of the idea of the good.

Besides arithmetic and geometry, Plato recommends for the curriculum of the leaders or "guardians" of society, the science of astronomy. In this case also, he makes perfectly clear that the educational value of astronomy is not in its fitness as a theoretical basis for calculating the observed positions of the stars, but as a path towards the recognition of the divine nature of the sun and planets.

We see that Plato valued all sciences as instruments for moulding the mind of the student in such a way that his conduct would conform to the educational and political goals of the government. His emphasis was on "goodness", not on "truth".

2. Are "Truth" and "Goodness" Compatible Criteria?

If the authorities in government and education require that a scientific doctrine be useful for the education of "good citizens", it seems probable that they may object to doctrines which have been established by using only the criteria of logical correctness and agreement with observation. However, if a doctrine is confirmed "by science", meaning by observations and logical conclusions, how can its social usefulness have an influence on its acceptance? If a scientific theory is "true", we can derive from it statements which can be checked by observation. These statements of fact are certainly determined by the theory. But, on the other hand, it is not possible to derive a scientific doctrine from observed facts. It is certainly possible that several scientific theories may account for the same observed facts. In this case, the "logico-empirical" criteria are compatible with two or more different theories. The authorities, state or church or public opinion, can select from among these doctrines the one which is the most useful for the training of "good citizens". To understand that the observed facts do not determine scientific doctrines unambiguously is the most important prerequisite for understanding the role played by sociological factors in the acceptance of scientific doctrines. Many people have claimed that there cannot be any influence of state or church on scientific doctrines because no authority can "change observed facts". This is certainly true. But an "authority" can require the abandonment or acceptance of scientific *theories* without requiring a "change of the facts".

The most conspicuous example of such a situation in history is the choice between the geocentric and the heliocentric world systems. When Copernicus advanced his heliocentric hypothesis, it was only a new way of deriving already known facts about the positions of the planets on the celestial sphere. The choice made by individuals was by no means dependent on their insight into astronomical research. It depended on their adherence to particular philosophical and theological creeds, or on their "common sense". Since no conclusion of astronomical science dictated an adhesion to one or the other world system, the authorities were able, without contradicting the results of "science", to enforce the choice of the geocentric system. Not only the Roman Church intervened in this way, but also, with the same energy, a number of the Protestant churches, particularly the Lutheran.

In this example, we see that the influence of "authorities" or, more generally, of social powers, of "existential factors" (Merton) on the acceptance of a scientific doctrine was made possible by a kind of "stalemate" between conflicting scientific doctrines.

A stalemate in the fight for "truth" usually results in the victory of the doctrine which is accepted in the competition for "goodness". But in this case, the firm establishment of Newtonian mechanics in the 18th and 19th centuries meant a clear victory for the "heliocentric, or Copernican, world system". Newton introduced the concept of "absolute space", and, according to the Newtonian mechanics, it was definitely certain that the earth was moving relative to this absolute space. This defeat of the geocentric system, in spite of its "goodness", was only made possible by the decline of the ecclesiastical authorities and the rise of liberal governments. The Copernican system had become a symbol of liberation from church authority.

Since the end of the 19th century and the beginning of the 20th, relativistic mechanics and positivistic philosophy have developed, both connected with the physicist, psychologist and philosopher, Ernst Mach. He clearly demonstrated that to say that "the earth is moving" or "the earth is at rest" are merely two ways of describing the same facts. The choice of one or the other is a question of simplicity of description. Since the Russian Revolution in 1917, the ruling party of the Soviet government has regarded with strong misgivings any possibilities of justifying the decision of the Church against the Copernican system. They believed that the relativistic physics and positivistic philosophy could be used to influence the mind of the people in favor of the Church and other traditional authorities. The Soviet government suspected a harmful influence on the conduct of its citizens. This is one of the reasons why positivistic philosophy (often called "Machism") has been ostracized in the Soviet Union. The Soviet government has had to accept the "observed facts" in relativistic physics, but it has required a non-positivistic interpretation. On the other hand, the quasi-justification of the Roman Church by the new philosophies has led to some "flirtation" between positivism and the philosophies favored by the Church.

We note that whenever a decision is left open by science, this decision is resolved by churches, governments, or public opinion, in short, by "existential factors".

3. MECHANISTIC AND VITALISTIC WORLD PICTURES

There is a similar stalemate in biology between the doctrines of "mechanism" and "vitalism". Adherents to the doctrine of "mechanism" claim that physico-chemical laws alone can account for the behavior of living organisms, while adherents to the doctrine of "vitalism" deny this and believe in specific "organismic" laws. When I spoke once with Bertrand Russell about these alternatives, he said, "This is not a scientific but a political problem". As a matter of fact, we can see very frequently that political or religious authorities take sides in this issue because they believe that the "organismic" or the "mechanistic" hypothesis supports their particular philosophies.

E. Krieck, who had the authority to define the official philosophy of education in Hitler's "Third Reich", said bluntly: "The world picture fitting to the Third Reich is the organismic world picture". He connected the "mechanistic" world picture with the political doctrine of liberalism. He said: "In the field of science, the liberalistic illusion, which consists in teaching an arbitrary aggregate without sense and coherence, will be terminated". (E. Krieck, *National-Politische Erziehung*, 1933)

The Soviet government, on the other hand, has strongly objected to "vitalistic and organismic" theories as being encouraging to the medieval tendencies in church and society. The most radical form of "vitalism" is the belief in telepathy, the belief that between two human beings interaction can occur without any physical communication. J. B. Rhine, the most prominent advocate of telepathy in this country, has suggested that we might undermine the political philosophy of the Soviet government by propaganda on behalf of the vitalistic conceptions of biology and, particularly, on behalf of telepathy. He writes (in the magazine *Tomorrow*, 1950):

> If a human being is only a system of physical fields of force, if there is nothing at all in his make-up beyond the properties of matter and the energies of physics, we may as well admit right now that the Russian ideology will win in the end, no matter what our military strength may be . . . .

Rhine, like Plato, the Roman Church, the Nazis and the Soviet government, recommends a scientific theory because it is a good instrument in shaping the minds of people in favor of a particular political creed. He recommends vitalism and telepathy as weapons in the political struggle against Russia. He writes, "It would be downright insane to ignore anything that promises to split the foundation of Soviet philosophy wide open". He even argues that "mechanistic biology" has to be rejected by all advocates of "free enterprise". To regard human beings as "mechanisms" is incompatible with a belief in "freedom of choice". How right Bertrand Russell was to call the "mechanism-vitalism" alternative a political problem, not a scientific one.

Since the 18th century, Newtonian mechanics or the interpretation of the physical world in terms of this theory has been the backbone of the mechanistic world picture. Around 1900, however, scientists ceased to regard Newtonian mechanics as an all-embracing theory of physical phenomena and recognized it as being valid only within a restricted domain. It is not applicable to high velocities or very small sizes. A tendency soon became noticeable to interpret this restriction of Newtonian mechanics as an opening wedge for an organismic and vitalistic biology and even for an organismic physics. The merely statistical character of the new laws was interpreted as indicating the existence of ''freedom'' in the vitalistic and organismic sense. A great many authors have believed that the new theory confirmed the kind of freedom and responsibility of the individual which traditional jurisprudence regards as a necessary basis for a just punishment of criminals.

Many educators, clergymen and judges have hailed this interpretation of science as a valuable support in the education of a ''good citizen''. As a necessary consequence, some pressure has been exerted by those authorities to encourage this interpretation.

Those who are not very critical take it for granted that modern science has ''proved'' the freedom of the will. On the other hand, there are some educators (I include under this category also religious and political leaders) who know approximately what it means to say that a scientific doctrine is ''proved''. It means that the general statements of this doctrine are compatible with the observed facts. In our case, the opposite statement seems also to be compatible. In a certain sense, one would say that from the scientific viewpoint the freedom of the will could either be accepted or rejected. We have here again the scientific ''stalemate'' that gives rise to the influence of what one may call ''moral pressure groups''. A very instructive example of this is given by the prominent philosopher of law and liberal judge, Jerome Frank. In his book, *Fate and Freedom,* he takes for granted that science cannot decide whether there is ''freedom of will'' or not. Now the educator comes in. Since, according to his opinion, belief in ''freedom of will'' supports the ''American Way of Life'', we have to make use of the leeway provided by science and accept ''freedom of will'' as a statement within our system of accepted truths. I could add a great many examples of a similar type.

There is a widespread opinion that the influence of religion and politics on science occurs only in strictly ''authoritarian'' countries. The Roman Church, the Hitler government in Germany, the Soviet government in Russia have been accused of making political considerations reasons for the acceptance of a scientific theory. My examples show clearly that the intention to use scientific theories as material for the education of good citizens is as active today as it has ever been. This intention will exploit every stalemate between conflicting scientific doctrines.

4. ''Real Things'' and ''Abstract Concepts''

A distinction has often been made in the general principles of science between concepts which denote ''real objects'' and other concepts to which no ''real objects'' correspond. The latter are referred to as ''abstract concepts'' or ''auxiliary concepts'' or ''constructs'' which ''exist only in the mind'' of the scientist. For example, there has been in philosophy a centuries-old dispute between ''atomism'' (or mechanism) and ''dynamism''. The first doctrine maintained that ''matter'' (divided into atoms) is a real thing while ''force'' is only an abstract concept. ''Dynamism'' asserted just the opposite. In the 19th century, the concept of ''force'' was replaced by ''energy'' and ''dynamism'' by ''energetics'', but the mediaeval dispute was continued under the new names. Similarly, in the theory of electromagnetism, there has been the dispute as to whether the ''electric charge'' is ''real'' and the ''field'' only an abstract concept or vice versa. In the 20th century, there has been the controversy whether the *psi* (Schrödinger) function is a ''real object'' or only an abstract concept.

A similar question has played a role in the foundations of Relativity Theory. This is whether ''space'' and ''time'' are real things or only auxiliary concepts which the scientist uses to bring order into his sense impressions.

In the social sciences, there has been an old dispute about whether the individual is a ''real thing'' while the ''nation'' is merely an abstract concept, or whether the ''nation'' is a ''real thing'' while the ''individual'' is merely an abstract concept. In this case, we see clearly that by saying that a certain concept denotes a ''real object'', we give it more importance than by calling it an ''abstract'' or ''auxiliary'' concept.

Professor C. J. Ducasse has pointed out frequently that the predicate ''real'' always means a predilection for the use of some concepts. We can easily find very drastic examples of the use of the terms ''real'' and ''abstract'' in the service of political doctrines.

Danislewski, a leading theoretician of the 19th century Slavophile movement in Russia, wrote that ''mankind'' is a product of "abstract generalization" while "nation" is a "culturo-historical type" and a product of "intuition and personal experience". Such statements have the clear purpose to degrade love for mankind and to extol nationalism. But they claim to be based on the results of social studies, anthropology and sociology.

We learn to understand the role of such general statements in the social sciences even better if we look at a statement written by Joseph Goebbels in 1927, six years before he became minister of propaganda and culture in the Hitler Government. Goebbels wrote:

> The nation is a part of mankind. Mankind is no thing in itself and the individual is no thing in itself, either. *The thing in itself is the nation* . . . The materialist regards the nation only as a means to an end and will not recognize it as a substance by itself. For him the nation is an intermediate step between man and mankind; mankind is for him the end . . . Therefore the materialist is necessarily a democrat. The idealist regards the concept 'mankind' only as an abstract concept. Mankind is something imagined only, not a fact.

In the last sentence, the predicate of "being a fact" can certainly not be confirmed by any comparison with observations. It actually means only that the author of the statement does not like to work for mankind and does not want others to participate in international work.

In a similar way, the predicate "thing in itself" is ascribed to a subject as if it were a property which could be confirmed by some kind of experience. Actually, it expresses only the author's predilection for the subject.

5. Are There Alternatives in Modern Science?

Many scientists like to point out that it does not actually happen that the observed facts can be equally well explained by different theories. They say that in any period there is one theory which is "accepted by the scientists". This is certainly not the case if we understand by "theories" a system of statements on the highest level of abstraction. Even in the field of mathematical physics and astronomy, we don't know whether the assumption of a finite universe or of an infinite universe checks better with observed facts. The same situation prevails in most questions of cosmology. All phenomena which are explained by the Theory of Relativity can be just as well explained by the existence of an ether. In the latter case, all changes in clocks, for example, are physical consequences of their motions relative to the ether. As for Quantum Theory, according to groups of scientists around Einstein and Schrődinger, it is not "proved" that the statistical laws of Quantum Theory are the most general principles from which the quantum phenomena can be derived. Perhaps these laws are just rules about the average behavior of particles. The final laws may be field equations which we have to average in order to obtain the usual Quantum Laws.

The situation is even more obvious in biology. We don't know whether the observed phenomena can be derived from the assumption of spontaneous generation or from the principle that life can only originate from life. If there is spontaneous generation, living organisms may originate from inorganic matter by pure chance. The question arises whether this is plausible or whether biology has to invoke some "plan" or "purposiveness" in the world by which changes can take place which are not likely to happen by "chance". This alternative "chance" or "purpose" cannot be decided by reference to observed facts or by "scientific research" in the ordinary sense of this word.

All the political doctrines which advocate a return to the political and social conditions of the Middle Ages show a definite predilection for one of these alternatives. They advocate "purposiveness" and reject "chance" as well as "mechanistic determinism". We mentioned in Section 3 that E. Krieck, the official spokesman of the "Third Reich" in education and philosophy, declared the "organismic world picture" to be the German world picture. According to him, "in the German universities only such men are entitled to teach and to do research who are committed with their whole personalities to the German world picture". (1933)

Oswald Mosley, the leader of the British fascists, writes in his book *The Alternative* (1947):

> The more intricate the pattern of the Universe, as demonstrated by modern physics, the harder it becomes to believe that the whole vast mechanism assembled itself by chance . . . We believe that it is now possible to derive from the actual evidence . . . some idea of the pattern of God . . . It is certainly clear that the purpose of this earth is a progressive movement from lower to higher forms.

Thus the choice between alternatives left open by science has been exploited for the justification of racial discrimination which helps the universe in its "progressive movement to higher forms".

If we consider the question whether the hypothesis of an "ether" should be abandoned in physics or not, we note easily that political influences have been active in bringing about a decision. In the first years of the Soviet government, the representatives of official Soviet philosophy denounced the Theory of Relativity as "idealistic" and "reactionary" because by dropping the "ether" the principle of "materialism" was dropped; that all observable phenomena have a material basis. On the other hand, the official spokesmen of the Hitler government in Germany denounced the Theory of Relativity as "materialistic". They claimed that the "ether" was not a strictly material substance and that Einstein had dropped the "ether" because he was a confirmed materialist to the degree that he was not willing to accept an "immaterial substance" like the "ether" as a basis of physical phenomena.

In the other cases of stalemate in scientific argument, political and religious pressures have also been brought to bear in favor of one or the other of the alternatives. Soviet philosophy has denounced the cosmological theory of a finite space, propaganda of organized churches has denounced spontaneous generation, advocates of "freedom" have denounced the strictly causal interpretation of Quantum Theory in order to clear a path for "freedom of will".

All this exploitation of stalemates in science makes a fascinating topic of research in the sociology of science which has been explored very little as yet.

6. "PASSIVE" AND "ACTIVE" POSITIVISM

We have learned that every stalemate reached in the application of the usual scientific method provides an opportunity of a choice between alternative hypotheses. This choice is "free" in that it is not determined by the advance of science in the ordinary sense. The existence of the "ether", for example, cannot be proved or disproved by the methods of physical science as practised by the professional scientist. We have also learned that this "freedom" of choice can be exploited in favor of social, religious or political creeds. For this reason, all groups who are interested in mustering "scientific" support for their creeds try to resolve such stalemates in science. The scientists are urged to admit their failure and to beg for help from philosophers or theologians. The declarations of the Holy Office in Rome, concerning the Copernican conflict, told the scientists that they could not do anything except collect observational material. But by this work, the choice between the two conflicting world systems could not be decided. Therefore, the scientists had to leave this decision to the philosophers and theologians. J. Maritain, who did a good job of making medieval philosophy palatable to the modern scientist, wrote bluntly that philosophy is dependent upon science only in the sense in which a lady is dependent on her chambermaid. In other words, philosophy takes only its raw materials from science but reaches its decisions on other grounds. Thus the Church took a view we would call today a ''positivistic view''. Scientists were warned to keep to the strictly logico-empirical method and to refrain from all audacious ''speculation'' which was to be reserved for ''higher'' types of knowledge. It is well known that the Church took the same stand, perhaps even in a more drastic way, with regard to the Darwinian theory of evolution. The advocates of the Church have recommended an extremely sceptical attitude to biologists and have advised them to draw a sharp line between observed facts and speculative thinking.

I must admit myself that I have learned a great deal from the anti-Galilean and anti-Darwinian writers who have tried to bolster up the cause of the Church. They have stimulated critical thinking and a clear-cut separation of speculation from facts. However, their aim has been to ''clear the deck'' for sustaining ''superscientific'' argument or ''metaphysical argument'' as it is called.

We notice a very similar attitude toward the sciences when we direct our attention to the National Socialist Government in Germany. The Government and the ruling party were concerned by the fact that generalizations had been developed in modern physics (in particular, Relativity Theory and Atomic Physics) which did not seem to match well with the political philosophy of the Third Reich. Therefore, the Government convoked in Seefeld (Tirol), November 1942, a conference in which leading physicists and political experts should develop a kind of compromise. The result was very similar to the compromise between the Roman Church and the adherents of Copernicus. Science had to be ''contained''; it was authorized to ''collect'' facts but not to interpret them (in a very general way). A resolution was passed in Seefeld according to which modern atomic physics was authorized as a description of observed facts under the condition that the scientists refrain from making any statements about the "constitution of matter". The underlying assumption was, of course, *that authentic knowledge about the "constitution of* matter" would be forthcoming from some "superscientific" source.

We can see that generally the powers that have been interested in controlling human conduct have envisaged an attitude of "humble positivism" among scientists. Scientists should simply collect material and leave the undecided questions of science to be resolved by the educators and rulers of men in such a way that the result will support an education for a "good life". But some scientists do not believe that "superscientific" methods of knowledge can answer the questions concerning which science has reached a stalemate. These scientists believe that where science has no answer, no one else has any answer, either. One has to wait until a scientific solution is reached. The decisions encouraged by social powers do not offer us any "truth". They are instruments of propaganda for a certain way of action. Whether one approves such an interpretation of science depends only upon whether one approves the way of life which is to be supported. This attitude is not "humble positivism" or "passive positivism" but we may call it "proud positivism" or "active positivism". This school of thought refuses to recognize the solutions reached by "superscientific" methods. It examines the decisions favored by social and educational influences and attempts to interpret them as acts which can be derived from the laws of human behavior.

7. Can Science Be Its Own Philosophy?

All the educational groups (in the widest sense of this word) who have encouraged and even cajoled "humble positivism" have rejected and flayed every attempt at "active positivism". The latter would frustrate all attempts to provide political and religious propaganda with the support of science. Jacques Maritain in his book *The Degrees of Knowledge* praises "humble positivism" because it teaches science to "know its place". But he attacks those scientists who have attempted to make of positivism, as Maritain puts it, a "philosophy of nature", or in the sense of August Comte, a "Positive Philosophy".

All of these groups have attempted to limit science to the collection of material and to enforce a "philosophical interpretation" which supports the "way of life" which they regard as a "good" one. For this reason, they will insist upon an autonomous philosophy that will provide decisions where science, in the narrower sense, has reached a stalemate. Organizations like the Roman Church or the Third Reich in Germany have denounced in the strongest possible terms the claims of "active positivism" or, as Maritain puts it, of "scientism" to discuss the high-level generalizations of science in scientific terms.

We find the same attitude toward "active positivism" in the announcements of the Communist Party in the Soviet Union. They require positively a philosophy which is to control scientific work and which can be controlled itself by "existential factors". This means, practically, control by government, church or public opinion. On the other hand, the autonomy of science is denounced. If we read the articles "Philosophy" and "Natural Science" in the official *Little Soviet Encyclopedia,* we find the following formulations of Russian Positivists emphatically denounced: "Philosophy overboard" (Minin); "Science is its own philosophy" (Boritchewski); and "Philosophy consists of the most general results of the natural sciences" (Stiepanov).

Thomistic philosophy which has become more and more the official philosophy of the Roman Church points out bluntly that there is a superscientific knowledge which is provided by philosophy. Msgr. Fulton J. Sheen says in his book *Philosophy of Science* (1934): "No science proves its first principles, but accepts them from a superior science, until finally one reaches metaphysics, which defends its own principles". One of the principal advocates of Thomism in the United States, Mortimer Adler, stresses the point that philosophy is independent of the changes in science. He writes in his little book *St. Thomas and the Gentiles:* "Philosophy is a body of knowledge, not attained by any of the specialized methods of empirical research, and consisting of truths which are not dependent for their validity upon the findings of science." Since obviously philosophy cannot advocate principles which contradict the observed facts which are found by science, Adler's statement can only mean that philosophy, as an autonomous body of knowledge, can reach a decision when and where a stalemate in science is reached.

There are many scientists who are willing to follow the advice to accept a "humble positivism" as their own doctrine and to leave higher generalizations to "educators" who reach their decisions by considering "philosophical implications". But there is a difficulty which is very often ignored by those who accept this view.

A. N. Whitehead in the introduction to his book on the Theory of Relativity points out that a scientist who tries to eliminate every kind of ''philosophy'' from his presentation of science will necessarily be influenced by the chance philosophy that he has imbibed as a child from his nurse or his Sunday School teacher or perhaps from the structure of his everyday language. Also, the average scientist frequently ignores this fact, but there have been quite a few prominent scientists who have emphasized that every presentation of science on a high level of abstraction is imbued with a certain philosophical interpretation which is not a result of scientific research. It may be a mechanistic or organismic or materialistic or an idealistic interpretation. The real meaning of these interpretations in physics or biology cannot be found in the sciences of physics and biology. But this fact does not imply that these generalizations reflect some superscientific knowledge. The real meaning of these generalizations can be found within science if we include also the social sciences in the system of the sciences. The idea of a superscience called philosophy or metaphysics arises only if we mean by ''science'' some special science instead of the system of all the sciences. The fact that no special science can, as F. J. Sheen said, ''defend its own principles'' does not lead to the conclusion that the system of *all* sciences cannot do so. The refusal to draw such a conclusion was the original idea of the ''Positive Philosophy'' as it was outlined by A. Comte and his disciple, J. Littré. They claimed that cooperation between the natural and social sciences would reach the objectives that were formerly reserved to philosophy. Their idea has been advocated more recently by men like J. Piaget and J. Dewey. John Dewey in his book *Reconstruction in Philosophy* (1920) presents the case for ''active positivism'' in a strong and impressive way:

> The history of philosophy will take on a new significance. What is lost from the standpoint of would-be science is regained from the standpoint of humanity. Instead of the disputes of rivals about the nature of reality, we have the scene of human clash of social purpose and aspirations.

INTEGRATION IN THE BIOLOGICAL AND SOCIAL SCIENCES

W. H. Sheldon
Department of Medicine, College of Physicians and Surgeons, Columbia University

As I pondered recently on what to say to a then imagined assemblage of intelligent integrators on the subject of integration in the biological and social sciences, it seemed somewhat of a pity that any need for such a discussion should appear to exist. For surely the essential oneness of biological and social science is sufficiently obvious. It is obvious, that is, if the interrelationships among human beings do in fact offer a proper field for realistic inquiry, or for that kind of unprejudiced inquiry which men have recently learned to address to the behavior and to the bodily functioning of their fellow beings, the other animals.

Most of us here today would readily grant that, to begin with at least, the proper study of man is biological man. For most of us the concept of a functional continuum extending from organic structure to observed behavior seems as natural when applied to the behavior of human beings as to that of other living organisms. Yet we are living in a society founded on basic tenets diametrically opposed to such an outlook, and the psychology underlying our institutions is not in fact biologically oriented at all. The rationalizations of life on which our current institutions rest are fundamentally mystical and supernatural, so perhaps it is not greatly to be wondered at that the species appears to have lost its way, in a biological sense, and to have drifted into an age of both hebephrenic and Dionysian confusion. The mixture of a basic philosophy of insolent supernaturalism with suddenly developing destructive power is a little like that other recently discovered bad mixture of alcohol with gasoline. The disastrous consequences are being felt in economic, sociopolitical and reproductive affairs as well as in religious and educational affairs, and we seem to be steadily approaching a major crisis. The crisis may be followed by smoother seas for a long season. It may be followed by social psychosis and chaos.

I think it has been growing increasingly plain that the situation calls for a biologically oriented psychology, or one taking for its operational frame of reference (its starting point) a physical anthropology expressed in terms of components (variables) which can be measured and quantified within the same terminological system at both the structural and behavioral ends of the structure-behavior continuum.

The general philosophy and plan of social control — the *humanics* resulting from acceptance of that sort of psychological outlook—might be called a *biological* humanics. It seems to me that the crux of your problem of integrating biological with social science is simply the lesser but more acute problem of bringing yourselves to accept fully, in your social philosophy, the implications of a biological humanics. I suspect that not a tenth of one percent of American biological scientists today, let alone the social scientists, have really faced these implications and have accepted the personal responsibilities they entail.

Perhaps the most direct way of putting the matter would be contained in two propositions: 1) that integration between biological and social science necessarily implies a continuity between methods used for studying structure and those used for studying behavior. 2) That our whole present structure of social science, ignoring as it almost always does the factor of describing taxonomically the structural variation of individuals, is like a house built on sand. In practice social scientists do not, and in particular sociopolitical leaders who are wielding power and influence do not, show much awareness of the implications of a biological humanics.

It may be that so far as the colleges and the sophisticate literature are concerned the battle for verbal acceptance of the idea of the biological evolution of man has been won. But the implications of the idea have scarcely as yet reached the general consciousness at all. We should not forget that only a generation ago there was still a somewhat vigorously expressed difference of opinion, notably in Dayton, Tennessee, regarding the historical relationship of man to his earth and to his fellow animals. Certainly the implications of a biological humanics were as foreign to the minds of the Bryan cohorts at that Dayton trial in 1925 as vegetarianism is to the mind of a healthy tomcat. For those people an integration of biological with social science would have been tantamount to an integration, in the mind of the tomcat, between a taste for young robins and a taste for stewed burdock leaves.

A true integration between biological and social science would seem to call for one of three alternatives: Either acceptance by social scientists of the meaning and consequences of a biological humanics; or a similar acceptance among biologists of the consequences of the theological humanics now partially prevailing in current ratiocinations of the social institutions and of current conventional social attitudes; or elaboration of some kind of formula of compromise. I should say that the philosophical history of the present century has been essentially a chronicle of abortive effort in the direction of the third alternative — attempted compromise. Space does not permit here any attempt on my part to summarize that story. I think it is a dismal story, in fact a shameful one, and I suspect it is possible that more harm has been done to the human cause during the past half dozen decades of abortive compromise (and consequent essential dishonesty) than during the preceding millennium and a half of predominantly honest theological insolence.

Our present institutions and the social attitudes contributive to them do not in fact rest on an honest theological humanics but on a vague compromise which represents ground almost certain to be abandoned as soon as a political focus (world order) is achieved. The philosophical choice will then be narrowed to the first two of the alternatives just now listed — either wholehearted acceptance of a biological humanics or a wave of hebephrenic jettisoning of human responsibility and another frantic scurrying back to some supernatural theology. This latter eventuality may happen before we know it. As a species we may have been growing biologically degenerate at a rate faster than anyone can believe, and we may suddenly scuttle to cover like a swarm of rats caught in a twinge of agoraphobia. It takes good men to face the implications of a biological humanics.

I suppose the term social science implies some degree of acceptance of responsibility for social control, not mere description or mere prediction. Certainly biological science carries that implication with respect to its own subject matter. Scientists are conative animals as well as cognitive and affective animals, and even for a scientist the purpose of interfering with a series of events is partially that of amplifying power over further events. Biologists however are usually in position to play down the conative side of their motivation, and to escape any consequences of it by proclaiming a remote disinterestedness. Social scientists ordinarily cannot do that and thereby they face a far more difficult situation. That is, they face it when they are both intelligent and courageous.

Social science is by its nature almost immediately *participative* in human affairs and controversies. Therefore social scientists have to face an almost immediate accounting for their decisions, their commitments, their expressed ideas. If therefore a social scientist offends the conventional mood in any way, and gets any attention, he is almost automatically under devastative attack. A biologist is less likely to be understood, and so has a better chance to survive a radical departure from the conventional bad habits of his time. This is an advantage the biologist derives from working with material one or two steps removed from the most immediate emotional concern of members of his own species. The biologist is in somewhat stronger position than the social scientist and can indulge himself in a greater degree of independence of thought and attitude. This may be why biologists have been largely resistant to philosophies patently absurd, such as those postulating wishful shortcuts to human salvation, even when the latter have been hysterically popular. In any case it looks as if the impulse for integrating biological good sense with the practical problems of social control will be more likely to find favorable soil among biologists than among the social science people.

All this is only common sense. What is more important, and far more often forgotten in our present culture, is that the "impulse for integrating" is itself a product of neither biological nor social science, but is religious in nature. We are facing an essentially religious difficulty and if we are going to solve it we are going to have to treat profound religious pathology. If the job is to establish the continuity between biological and social science major attention needs to be given to the pathology which tends to break that continuity. Now my diagnosis of the pathology is this: The continuity between biological and social science appears to be broken wherever the oneirophrenic proposition is entertained that bodily structure could be one kind of thing while mind or temperament or the unconscious or the genetic pattern could be something else independent of it or out of the continuum with it. In short, it is the mind-body idea, or the psychosomatic idea, that appears to lie at the bottom of religious pathology.

I am not concerned with denying the possibility of the truth or ultimate rightness of any dualistic hypothesis. Indeed I suppose that ultimate truth may well reside in that quarter despite my hope to the contrary. What I do deny is the pragmatic usefulness of such an outlook. It seems to me a schizophrenic and a biologically unhealthful business, and it leads to postulation in the human behavioral hierarchy of causal entities that are outside the natural law. These "wild cards" destroy the integrity of the game, and they greatly hinder the development of a natural history of mankind. It is as a naturalist, not as a philosopher, that I protest against loading the deck with wild cards, for I love the order and system of nature and can see no reason for departing from it in our consideration of the human species.

At the Constitution Laboratory we make the philosophical assumption that human behavior, whether physiological, hormonal or mental, is always structure in action. In so doing I suppose we follow Vaihinger's philosophy of *Als Ob,* although reversing the premise. You will recall that Vaihinger suggested we proceed *as if* Mr. G were running the show and were carrying the responsibility for it. As a way of life that proposition offers many ameliorations. Particularly, it turns the naturalists and the biologists out of the temple and directs their attention away from the dignity and nobility of the human estate, for human ethical affairs are then exempt from the curiosity of naturalists and are Mr. G's business. More especially, the human body can then be somewhat disregarded and the quality of mind or soul, stemming as it does directly from Mr. G, is most emphatically *not* a reflection of the structure of the body or of the quality of the gut. Indeed, the continuity between biological and social science is broken altogether.

Vaihinger was perfectly realistic about ultimate truth. Admittedly he had a somewhat irreverent or possibly unchastened way of referring to Mr. G as God, thus calling the deity too familiarly by his first name, but he had no illusion of ultimate or immediate knowledge of Mr. G. He merely liked the idea of avoiding the task of building a biological or naturalistic foundation for social science, and he preferred to live *as if* Mr. G had already worked out the problems of social science.

That in fact has been the prevailing and I think insolently unhumble social philosophy of our comparatively recent past. At the Constitution Laboratory we are at least more humble, and we call Mr. G Mister. Everybody else is Doctor, which is German for daughter (when gutturally pronounced), is French for teacher, and has numerous other well-distributed meanings in Webster's unabridged dictionary. These include 1) a learned man, 2) a philosopher, 3) a physician, 4) a wizard in a savage tribe, 5) a prong-tailed skate, which is a fish, 6) a donkey engine, 7) a device for polishing a seat (presumably a swivel chair seat), 8) a pump in a river steamer, 9) a cock, 10) a loaded die for shooting craps, 11) a fixer of anything, 12) an artificial fly. The term Doctor can thus be safely applied to all women and to nearly all men, while Mister is reserved for the most extraordinary aristocracy.

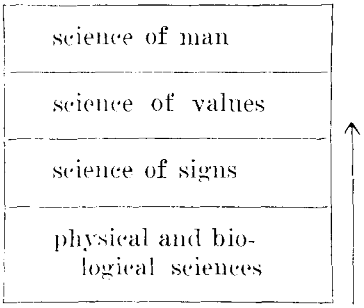
Meanwhile it will be remembered that Occam, early in the fourteenth century, adopted a view directly opposed to that of Vaihinger. Like the latter he laid claim to no mystical knowledge of ultimates, but in view of this admitted ignorance he felt that the way of choice was to live and think *as if* no mystical ultimates were likely to interfere with our human show, or were likely to be interested in it. *"Entia non sunt multiplicanda praeter necessitatem,"* he said, and this cryptic suggestion, which is Occam's statement of the Law of Parsimony, has been called Occam's razor. Entities (hypotheses) are not to be multiplied beyond necessity.

I suppose that one of the most urgent needs confronting the human species is the need for recording the *overt structure* of living individuals in such a way as to make possible a more systematic and more fruitful study of structure-in-action, which is actually the subject matter of chemistry, physiology and medicine and must become (if we are to attain the continuity we seek) the subject matter also of psychology and the social sciences. Perhaps if we elect to proceed philosophically *as if* it were known, a thing given, that organic structure in action is personality (also temperament, mind, and in fact behavior whether conscious or unconscious), if we can resolve as philosophers to operate on the basis of that drastic *Als Ob* assumption, perhaps we can even yet repair the broken continuity between biological and social science. My dark presentiment is that only if we can do it will the great schismatic tragedy of this age be overcome and final human disaster for the time being averted.

THE SCIENCE OF MAN AND UNIFIED SCIENCE

Charles Morris
University of Chicago

In 1939, at the Fifth International Congress for the Unity of Science, held at Harvard University, I read a paper with the title ''Semiotic, the Socio-Humanistic Sciences, and the Unity of Science.''[1] This paper suggested that ''the theory of signs (semiotic) furnishes the key to the incorporation of the socio-humanistic sciences in the structure of unified science.'' The proposal made was to develop the theory of signs upon a biological base, to use this theory in the construction of a biologically based theory of value, and then to approach the whole domain of man's cultural activities in terms of semiotic and axiology so constructed. In this way it might be possible to build a bridge between the natural sciences and the science of man, both of which would then fall within a single system of knowledge. Such a program may be represented in the following diagram:



In the last decade much scientific work has been done which is relevant to the carrying out of this program. It is these developments which I wish to comment upon at this time.

The Science of Signs

The speaker's book, *Signs, Language, and Behavior*, published in 1946, was an attempt to show that semiotic could be given a biological foundation such that scientific methods could be applied to the whole domain of meaning. The suggestion was made that the analysis of any sign might be given by stating the things to which it directs behavior and the behavior which it prepares to these things. The interpretant of a sign (the effect of the sign on an organism) would then be a disposition to react to something or other in a certain way, and the signification of the sign would be given by specifying the reaction thus prepared and the things to which the reaction is prepared. On this approach the basic terminology of semiotic would be constructed upon terms of the physical and biological sciences, and in principle the whole domain of meaning would be open to study by scientific methods.[2]

In this book it was further argued that such a behavioral approach to semiotic would allow a proper place for formal logic and logical analysis, since the approach itself furnished the basis for a distinction between analytical and non-analytical sign combinations. Symbolic logic could then be conceived as the analytical portion of semiotic. And since logical analysis is not a theory of meaning but an analysis of given or entertained meanings, it does not itself decide between various theories of meaning; hence it is compatible with a biologically grounded semiotic. Further evidence for this compatibility may be found in the work of Charles Peirce (especially volumes II and V of his *Collected Papers*) since Peirce too ultimately viewed the interpretant of a sign as a disposition to respond; in Book II of C. I. Lewis' *An Analysis of Knowledge and Valuation*, which grounds the ultimate distinctions needed by symbolic logic upon this Peircean view of meaning; and in Hans Reichenbach's *Experience and Prediction*, which finds no conflict between the pragmatist's theory of meaning and the analyses made by the logical empiricists. Here at a critical point in theory the approaches of the pragmatic and logistic wings of the unity of science movement seem to have converged through the development of a study of meaning.

Among the many steps toward a scientific semiotic which are taking place today, three may here be noticed. The basing of linguistics upon the theory of signs, as in the work of Roman Jakobson (see his *Sound and Meaning*), locates linguistics within semiotic, and this, taken together with the similar movement in respect to formal logic, augurs closer cooperation between the linguist, the logician, and the semiotician. Hans Reichenbach's attempt in *Elements of Symbolic Logic* to apply symbolic logic to the analysis of the everyday language is an instance of the direction of such cooperation.

The application of quantitative methods to content analysis by Harold D. Lasswell and his co-workers, while at present restricted to political discourse, is of sufficient generality to provide a model for the quantitative analysis of all types of discourse (see *Language of Politics,* by Harold D. Lasswell, Nathan Leites, and associates). Content analysis, so conceived, falls within the empirical or descriptive part of semiotic, and makes more precise its methods of study.

The elaboration of the theory of communication by mathematical, physical, and engineering techniques (as in Norbert Wiener's *Cybernetics;* Claude Shannon and Warren Weaver's *Mathematical Theory of Communication*) is further evidence that semiotic has now been linked to the basic scientific and technological disciplines. It is possible that the concept of feedback mechanisms stressed by Wiener and his associates may provide, in the form of circular neural processes, an approach to a fundamental problem of semiotic: the nature of the interpretant of a sign.[3]

THE SCIENCE OF VALUE

A central stimulus toward building a science of value on a behavioral basis has been the work of John Dewey. In his *Theory of Valuation*[4] he envisaged the task of the theory of value as the study of the nature and relation of prizing and appraisal. In his article "The Field of 'Value'", in *Value: A Cooperative Inquiry* (edited by Ray Lepley) Dewey uses the term "selective-rejective behavior" to characterize the general field of behavior within which the study of values is to be carried on; and in this article he maintains that an appraisal (or evaluation) does not differ in kind from a statement, and hence is controllable by scientific methods. In an article in the same book, following the general direction set by Dewey's work, I suggested that axiology might be regarded as the science of preferential behavior. Since a study of such behavior would of course include the way it affects and is affected by signs, this characterization of axiology embraces the study of preferences (prizings, selection-rejections), appraisals (evaluations, judgments of value), and their interrelations. So conceived, the field of value is open to scientific study.

As examples of studies of prizings in line with this approach reference may be made to the monograph by Henry A. Murray and Christiana D. Morgan, "A Clinical Study of Sentiments";[5] and to the various papers by Jerome S. Bruner and his associates.[6] For some years the speaker has been making a study of the preferences of various kinds of persons in various cultures. While the study has been mainly concerned with preferences among various possible ways to live, some attention has been given to preferences as to paintings and philosophies, and to the interrelation of these three sets of preferences. It is possible in this way to isolate empirically various value-patterns, to ascertain their number, to study their relative strengths in various cultures, and to investigate what kinds of persons accept which patterns under what conditions. The detailed results of this study have not yet been published, but an indication of the methods and the type of results can be found in an article "Individual Differences and Cultural Patterns" in the volume *Personality,* edited by Clyde Kluckhohn and Henry A. Murray, and in my book, *The Open Self.*[7]

The empirical study of appraisals has been slower in getting under way, since it required a theory of signs sufficiently developed to handle such signs and to compare them with other kinds of signs. It is my belief that the analyses given in *Signs, Language, and Behavior,* in Book III of C. I. Lewis' *An Analysis of Knowledge and Valuation,* and in C. L. Stevenson's *Ethics and Language,* have gone a considerable way toward meeting this need, and hence to prepare the ground both for the scientific study of appraisals and the scientific making of appraisals.

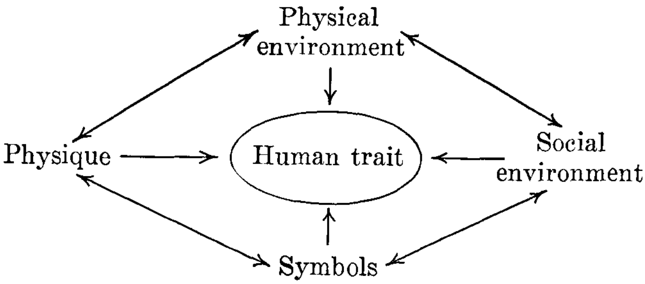
The situation seems to me to be something like this: appraisals are signs the evidence for whose applicability is in the last analysis found in prizings. As such, appraisals are cognitive (true or false) since they make predictions which can be tested; but they differ from certain other kinds of signs whose applicability does not involve the occurrence or non-occurrence of prizings as evidence. It is true, as Stevenson maintains, that conflicts of prizings are not necessarily resolved by the increase of knowledge; it is even true that a certain diet or way of life good with respect to some persons may be bad with respect to others. But these facts force us only to qualify our appraisals with respect to certain persons under certain conditions, and to admit that knowledge of values does not insure their attainment; they do not force us to abandon the view that appraisals are empirically meaningful and capable in principle of being controlled by scientific methods.[8]

If prizings and appraisals are subject to scientific study, then so is the problem of their relation. It is possible to investigate the influence of certain prizings in the formation or acceptance of appraisals, and the reverse effect upon prizings of appraisals formed by an individual or known to be held by other individuals. Similarly, the relation of prizings and appraisals to non-evaluative knowledge is open to experimental determination. My study of paintings has touched upon these matters, but serious work on such problems is yet to be done.

It is often maintained that preferences are not values, and that consequently a study of preferences has nothing to do with the study of values. To admit the first part of this thesis does not, however, require an acceptance of the conclusion. It is true that when we ascribe value to something we often mean that it is value-able, i.e., able to support a prizing, or able to support continued prizing, or more able to do either of these than is something else. These are, to be sure, claims made about an object, and not a description of existing prizings. But since they are claims to *prizability,* they can only ultimately be tested by observing whether, under stipulated conditions, the objects are prized or continue to be prized. So without claiming that preferences and values are the same thing, it is possible to maintain that axiology, conceived as the study of preferential behavior, is a study of values. I, indeed, know of no other way that values can be studied.

THE SCIENCE OF MAN

We must now note a third growing tendency in the scientific study of man: the tendency to regard any particular human trait as a function of many factors or variables, i.e., to think in terms of fields. This attitude contrasts with the tendency to look at man only in terms of physique or environment or culture or language. Each of these approaches has often been carried on in isolation, and indeed in competition with the other approaches; now we seem to have come to the point where we must realize that any of these approaches when carried far enough involves all of the others.[9] The resulting point of view may be crudely indicated in a diagram. ("Symbols" in this diagram stands for signs produced by an individual and affecting their producer.)



The trait to be studied may be anything whatsoever: clothes worn, philosophies accepted, schizophrenia, aggressiveness, literary preferences, and so forth. In studying this trait in individuals one seeks to find the conditions of the field under which the trait appears. Thus if one is studying schizophrenic traits from this point of view one would try to find out what types of organisms, in what environments, under what social relations, and equipped with what symbols, become schizophrenic. In seeking to uncover such laws, many individuals would have to be studied and many factors considered. The methods of factor analysis developed by L. L. Thurstone (in his *Multiple Factor Analysis*) furnish one important way by which the complex interrelations of such data can be handled.

The field approach to the study of man provides the context in which the science of signs and the science of values are themselves to be carried on. These sciences, conceived as behavioral sciences, are linked in the ways previously indicated with the biological and physical sciences. Putting them within the field approach brings out this relation in another way, for this approach requires that data on physiques and physical environment be included in the study of human traits. The science of man is in this way linked methodologically with the biological and physical sciences to form a single scientific system.

One of the merits of William H. Sheldon's important work (as reported in *The Varieties of Human Physique, The Varieties of Temperament,* and *The Varieties of Delinquent Youth*) is that his constitutional emphasis has forced us to take account of the biological differences in the individuals whose traits are being studied. He has supplied constitutional data on many of my subjects, and at a later time detailed reports will show how this research on psychoses, ways to live, and paintings in various cultures has confirmed his basic thesis as to the importance of considering differences of physique in the study of human beings. Here it is only necessary to add that Sheldon's work is of significance in helping to bridge the gap between the biological and the human sciences by reinserting the physique into the total field. The same service with respect to environmental factors has been performed by Ellsworth Huntington, whose life work was brought to a focus in *Mainsprings of Civilization.*

The constitutional and environmental factors link the science of man with the biological and physical sciences. At the same time, the recognition in personality development of the role of social factors and the role of symbols which individuals produce, ensures us that nothing distinctively human will be lost. The science of man is linked with the biological and physical sciences but not reduced to them. For as sign processes the ideas and ideals of individuals enter as formative factors in the total field; they are the way in which man in a genuine sense is one factor in the making of his own future, transforming his body, his physical environment, and his culture.[10]

CONCLUSION

Such are some of the ways in which the work of the last decade has advanced the program of integrating the science of man and bringing it within the framework of unified science. Further and rapid developments in this direction may be expected in the next decade. Such developments will not be of scientific interest alone; they are of importance for the integration of man as man. They help to destroy at the root the opposition of man's scientific and humanistic concerns. In integrating science we at the same time integrate and liberate ourselves.

NOTES

[1] An abstract of this paper was printed as part of the article "The Significance of the Unity of Science Movement," *Philosophy and Phenomenological Research*, 6, 1946, 508-515.

[2] References to criticisms of this approach, and my reply to these criticisms, may be found in "Signs About Signs About Signs," *Philosophy and Phenomenological Research*, 9, 1948, 115-133.

[3] See the papers by Warren S. McCulloch and Walter Pitts, *Bulletin of Mathematical Biophysics*, 5, 1943, 115-133; 9, 1947, 127-147. Also, D. O. Hebb's book, *The Organization of Behavior*.

[4] *International Encyclopedia of Unified Science*, vol. II, no. 4.

[5] *Genetic Psychology Monographs*, 32, 1945, 3-149, and 153-311.

[6] Such as "Value and Need as Organizing Factors in Perception," *J. of Abnormal and Social Psychology*, 42, 1947, 33-44; "Personal Values as Selective Factors in Perception," ibid., 43, 1948, 142-154.

[7] An article, "Comparative Strength of Life-Ideals in Eastern and Western Cultures," is to appear in a volume of proceedings of the second East-West Philosophers' Conference, to be edited by Charles A. Moore. [This volume has since been published under the title *Essays in East-West Philosophy*; Honolulu, University of Hawaii Press, 1951.]

[8] This issue is of such basic importance that it deserves to be made a central topic of discussion by the Institute for the Unity of Science. Philipp Frank's position in *Relativity—A Richer Truth* seems in accord with the position taken; but I am not sure that Reichenbach or Stevenson would agree.

[9] This point of view provides the basis of organization of the anthology *Personality,* edited by Kluckhohn and Murray. Murray has sometimes called this the "multiform method." Laura Thompson had come to a similar methodological position as a result of her work on the Hopi Indians; see her book *Culture in Crisis.* I had been led to the same result in the study of signs and values. This view facilitates the integration of the science of man, including therein the social sciences. A comprehensive attempt at such integration is found in the forthcoming volume *Toward a General Theory of Action,* by members of the Harvard Department of Social Relations and their collaborators. This book will appear in 1951 or 1952, Harvard University Press, with Talcott Parsons as editor. The point of view is an extension and elaboration of that found in Talcott Parsons' *The Structure of Social Action.*

[10] See *The Open Self;* G. H. Mead's *Mind, Self, and Society;* F. S. C. Northrop's "Ideological Man in His Relation to Scientifically Known Natural Man" in *Ideological Differences and World Order,* of which he is the editor.

PART 2

THE "MEANING" OF SCIENTIFIC STATEMENTS

THE VERIFIABILITY THEORY OF MEANING

HANS REICHENBACH
University of California at Los Angeles

I

In 1923 I was asked by the editors of the "Handbuch der Physik" to write an article about the philosophy of physics. Since one of the points I wanted to discuss in this article was the reality of the physical world, I used the occasion of a trip to the Bavarian mountains to see Moritz Schlick at his summer vacation place and to talk with him about this problem. I told him that I had recently discussed these questions with a young philosopher who was writing a book entitled "Der logische Aufbau der Welt", and who advanced the idea that Mach's positivism could be given a strict logical form by regarding any statement about the physical world as logically equivalent to a combination of statements about sense data. I myself objected to this equivalence; I thought that only a probability relationship could be maintained. Schlick supported me in this view; he argued that the positivistic equivalence was untenable.

That was in 1923. A few years later, after Schlick had invited Rudolf Carnap to become a member of his department and had succeeded in attaching Ludwig Wittgenstein to his circle in Vienna, Schlick changed his views completely and turned positivist. In this environment, those views were elaborated which culminate in the thesis that the meaning of a sentence is the method of its verification. Mainly as the achievement of Carnap,[1] the verifiability theory of meaning was thus given an elaborate logical form; for the first time in its long history, which goes back to William of Occam and Leibniz, this theory was set forth with the precision tools of modern symbolic logic.

My article written in 1923 had to wait for publication until 1929, when the volume of the Handbuch finally came out.[2] My refusal to accept the equivalence interpretation was based on the distinction between a complex (Gebilde) which is "artgleich" to its elements from a complex which is "artverschieden" to its elements, a thesis which I took over into a later book of mine as the distinction between a reducible and a projective complex of elements.[3] But I knew that it was vain to go into a further elaboration of my conception before I had been able to construct a theory of probability, since the concept of probability was fundamental to any theory of knowledge. Thus I concentrated my work on the analysis of the calculus of probability and its applications to physical reality, a subject on which I had been working since 1914.

After I had given to the problem of probability and induction a solution[4] which I regarded, and still regard, as satisfactory, I published in 1936 a version of the verifiability theory of meaning[5] which referred to the views of the Vienna circle but differed from it in several points. First, it substituted probability relations for equivalence relations and conceived of verification as a procedure in terms of probabilities rather than in terms of truth. Second, it abandoned the program of defining "the meaning" of a sentence. Instead, it merely laid down two principles of meaning; the first stating the conditions under which a sentence has meaning, the second the conditions under which two sentences have the same meaning. A rule of the latter kind was sufficient for all practical uses of language; and once the equivalence between indirect sentences and observation sentences was abandoned, an explicit definition of the meaning of indirect sentences in observational terms could no longer be given.

Shortly afterwards, Carnap presented the first exposition of his revised views on meaning. He had seen that the equivalence interpretation could not account for the structure of the language of science and proceeded to lay down conditions of meaning rather than give a definition of the meaning of an indirect sentence. This interesting paper on Testability and Meaning[6] marks the beginning of a new period for all those investigations which emanate from the positivism of the Vienna circle. The thesis that the meaning of a sentence is the method of its verification was abandoned. Carnap's reduction chains allot meaning to a term for the case that certain physical conditions occur, without supplying an explicit definition of the term. Thus what I call the first principle of the theory of meaning was here carried through in a form different from mine. It seems to me, though, that Carnap's treatment of meaning calls for a supplementation by some version of my second principle, since otherwise terms which we all would call meaningless could be adjoined to meaningful terms and thus acquire a status in scientific language. For instance, instead of defining the term "there is an electric current in the wire" by means of reduction sentences, we could add to this sentence, following Hempel's example, the invidious sentence "the absolute is perfect", thus including it into the accepted terms of language. Such adjunction is easily dealt with if a principle is introduced according to which the conjunction of the original with the invidious sentence does not have any more meaning than the original sentence alone.

In his further investigations, Carnap saw that his degree of confirmation, first introduced in the paper mentioned, requires a deeper elaboration through a theory of probability. The views which he has developed in this field differ essentially from mine. I suppose that his theory is intended to overcome the shortcomings of his reduction chains, which are applicable only in cases where probability relations can be practically replaced by logical implications, and which thus are too primitive instruments for the construction of scientific language.

Among former members of the Vienna circle, it was, especially, H. Feigl and C. Hempel who collaborated in adjusting the theory of meaning to a realistic language. Feigl's attempts in this direction go back to the early days of the Vienna circle; he had always been the exponent of realism within this group, and he has recently laid down his views in two excellent articles.[7]

II

After this short historical account, I would like to give a brief analysis of the verifiability theory of meaning in its present status, underlining those points about which there exists agreement among the various adherents of the theory, and setting apart those points about which there is disagreement.

(1) The verifiability theory of meaning lays down rules for the construction of meaningful expressions. These rules are conventions determining the structure of language. Being rules, they are neither true nor false, but volitional decisions. However, it is possible to make cognitive statements about the properties of the language resulting from the acceptance of these rules. These statements have the form of implications: if this convention concerning meaning is accepted, then the language thus resulting has such and such properties. It is possible to study such relations for various definitions of meaning and to compare the various languages. If finally one convention concerning meaning is accepted, it is possible to justify the decision for this set of rules in terms of a certain aim; for instance, the aim of interpreting the language of physics, or the aim of constructing a language that can be used for the purpose of human action.

On this point there is general agreement among the adherents of the verifiability theory of meaning. The problem of meaning falls under the category of what Carnap has called *explication.* A term of conversational language, so far used in a vague sense, is replaced by a precise term. The original term is called *explicandum;* for the term proposed to replace it I will use the name *explicans* (the term "explicatum" used by Carnap is misleading because of its grammatical form, which means "*what is explained*", whereas "explicans" means "what explains"). An explication can never be called true; however, it can be justified. That is, it can be shown that the explicans has properties which make the use of the term compatible with human behavior in connection with it.

(2) The verifiability theory of meaning concerns *cognitive* meaning. It therefore is not concerned with the meaning of imperatives, in which category value judgments are included, though they often do not have the grammatical form of imperatives. It is possible to correlate to imperatives a cognitive statement; for instance, to the imperative "shut the door" the cognitive statement "the speaker wants the door to be shut". Imperatives may therefore be said to have a cognitive component, which is taken care of in the verifiability theory of meaning. This is not meant to say, however, that the question of the meaning of imperatives is completely settled by the transition to the cognitive component. On this point, too, there is, I think, general agreement among the adherents of the verifiability theory of meaning; the theory concerns only cognitive meaning.

(3) Cognitive meaning is defined by a step process. First, a basic class O of *observational sentences* and terms is introduced which are assumed to have *direct meaning.* We may also speak of *primitive meaning,* i.e., a meaning which is not under investigation during the analysis to be performed. Sentences and terms of the class O may also be called *direct sentences,* or *direct terms,* respectively. Second, the meaning of further terms and sentences is constructed by the help of *derivative relationships D,* which connect these new terms with the basis O. A step process of this kind is assumed in all forms of the verifiability theory of meaning. The emphasis on this step process and the analysis of the construction of indirect terms is one of the major contributions of the Vienna circle to the theory of meaning.

(4) The properties of the basic sentences O must be stated more explicitly. It is sometimes said that we must know how to

verify them, and that this is the same as knowing their meaning; in other words, that for the basic sentences at least, the identity of meaning and method of verification can be assumed. But the statement "we must know how to verify them" is not very precise and open to criticism. Suppose a man is given a list of report sentences (Protokollsätze) which are not his own reports, but which he has good reasons to believe to be true. Can the man, whom I will call the reconstructor, construct the system of knowledge resulting from this list of sentences? Obviously it does not suffice him to know that the sentences are true; he must *understand* the sentences. This qualification can be made clear as follows. Suppose the report sentences are written by the use of the logical symbolism. Now if the two sentences "Peter is tall" and "John is tall" are written down in the list, it would not suffice if they are symbolized by the letters "p" and "q". They must be symbolized as "$f(x_1)$" and "$f(x_2)$", since otherwise the usual inferences, for instance, an inference of induction by enumeration, cannot be drawn. This shows that the structure of the report sentences must be known. That is, the report sentences must be symbolized, not in the calculus of propositions, but in the calculus of functions. A further distinction must be added. Even if the report sentences are given in their full structure, the reconstructor does not know to which of his own experiences the symbols "f", "x_1", etc., refer. In spite of the absence of such knowledge, he is able to follow all the inferences made for the construction of scientific theories, and indirect terms thus acquire for him a structural meaning. But what is still missing is the *relation to his own experiences. For this purpose, a final step is* required. The reconstructor must be given the interpretation of the empirical constants "f", "x_1", etc.; then all terms, including the indirect ones, acquire for him an interpreted meaning. For these reasons, the formulation "the person must know how to verify the sentences of the basis O" is better replaced by the formulation "the person must know the meaning of the sentences of the basis O".[8]

For the basis O, the following kinds of sentences have been used:

(α) reports about the objects of our personal macroscopic environment (concreta) at a certain moment

(β) reports about the same kind of objects, but including our past observations

- (γ) reports about the same kind of objects given by any human observer at any time
- (δ) reports about sense data at a certain moment (or, as in β or γ, extended to include the past, or different observers)
- (ε) reports about atomic physical occurrences, like coincidences of electrons, etc.

As to the choice of these bases, which were discussed at greater detail by Carnap and Neurath, there seems to be general agreement that the basis α has a psychological priority. The objects of the other bases then are regarded as constructed by inferences starting from the basis α. Doubts have been uttered whether the basis α is large enough; in fact, it is difficult to show how on that basis the statement that Caesar was murdered in 44 B.C. can be constructed. It seems we have to include in the basis our recollection of established laws, such as the law that looking into a suitable reference book you find reliable historical data about Caesar; but these recollections are mostly only potentially there. Who would be willing to include the unconscious in the basis α? A clarification of this point is desirable, although I believe that it involves no difficulties of principle.

(5) I will now turn to the derivative relationships *D*. There is today, finally, agreement among most of us that these relations cannot be equivalences between indirect and direct sentences. This widening of meaning is necessary in order to admit as meaningful such sentences as "the planets will go on traveling on their orbits after the death of the last human being".[9] Furthermore, it is agreed that the relations *D* cannot be merely deductive relations from the indirect to the direct sentences. The relations must transfer truth in the opposite direction: the basic sentences must confer a degree of truth-character upon the indirect sentences. This requirement means that the indirect sentences must be so constructed as to be appropriate for the derivation of observable predictions, i.e., of future direct sentences which we have good reason to treat as true before they are directly verified. This cautious formulation will indicate that the derivative relations *D* involve inductive inferences and that such inferences cannot be proved to lead to true conclusions, or even to a large number of true conclusions.

Now we all know that the interpretation of probability and induction is highly controversial today; in fact, the derivative relations mark the point where the controversy about probability enters into the domain of the verifiability theory of meaning. My own interpretation of probability and induction is so constructed that it satisfies the above requirement; this is admitted, as far as I see, even by my opponents, with the exception of Bertrand Russell, who claims that my inductive posit "is admitted to be not intellectually justified".[10] I would like to know where he found such an admission in my writings. The objections which other writers have raised against my theory of induction and probability maintain that the frequency interpretation of probability is unsuitable for the reason that it does not express what we really mean by a probability referred to a single case, or to a scientific theory. I answer that we are concerned here with an explication, and that for an explication of probability it does not matter what a man means. The explicans given by me has the property of making a man's behavior justifiable whenever he is in a situation which, in correspondence with established usage, he describes by means of the term "probable"; or in other words, it has the property of making the use of the term "probable" compatible with human behavior in connection with the term. This is a sufficient reason to accept my explication.

If others suggest other interpretations, they would have to give a similar proof for the explicans suggested by them. As far as I see, none among the advocates of other interpretations has even tried to give such a proof. Carnap regards his interpretation of probability as justified because, as he says, it is in agreement with the beliefs which a scientist has when he makes inductive inferences.[11] I do not think that this psychological brand of justification is acceptable. Scientists often have strange beliefs, and make fallacious inferences with good results. The logician is not interested in copying the scientist's mistakes. What he wants to give is a rational reconstruction of scientific inference, and for a reconstruction of this kind, correspondence to beliefs is no criterion of adequacy.[12] The only way of showing that a reconstruction of scientific inference is rational is to give a proof that the explicans logically validates the inference. It would be too much to ask that the conclusion be true; the cautious formulation above given is meant to explicate the term "validate". I was glad to see that Carnap now admits that a justification of induction must be given, and that he does not share Schlick's views according to which the justification of induction concerned a pseudo problem. But I cannot regard his interpretation of probability as justified unless he gives a proof that it is advisable, in some sense, to use his probability values as a directive for action.[13] From the presentations of his theory which he has published I would infer that such a logical justification can never be given, because he introduces a probability metric *a priori*.[14] But if the scientist should have to put up with an interpretation which makes his inferences unjustifiable in the logical sense, he could as well quit working.

(6) After defining the basis O and the derivative relations D, the term "verified" can be defined as "being derived from the basis O in terms of the relations D". Now there is general agreement that the condition of meaning is not actual verification, but possible verification. This widening of meaning is necessary in order to admit as meaningful such statements as "it snowed on Manhattan Island in the year 4 A.D.". Therefore the term "possible" must now be defined.

Three kinds of possibility must be investigated: logical, physical, and technical possibility. The first means non-contradictory character; the second, non-contradictory to empirical laws; the third, being within the reach of known practical methods. That a verifiability defined in terms of technical possibility makes the definition of meaning too narrow is now generally admitted. Schlick, and with him most members of the Vienna circle, have used logical possibility. But a definition of meaning in terms of logical possibility of verification makes the definition of meaning too wide, at least, when the interpretation of physics is concerned. For instance, Einstein's principle of equivalence, according to which being in accelerated motion means the same as being in a gravitational field, presupposes a definition of verifiability based on physical possibility. For these reasons I have advocated a definition of meaning in terms of the physical possibility of verification. A suitable definition of this kind of possibility and of physical laws is given in the frame of my theory of nomological statements.[15]

Since meaning is a matter of definition, it must be kept in mind that none of the three definitions resulting from the three kinds of possibility can be called "true"; moreover, none of them seems to supply the only suitable explicans of meaning. It appears that all three of them are actually used. The physicist usually assumes *physical meaning*, for which verification is physically possible. But in his discussions of physical theories which he wants to prove false, he often uses *logical meaning*, for which verification is logically possible. For instance, an absolute time can very well be defined by speaking of the logical possibility of signals faster than light; and when the physicist says that the theory of absolute time is false, he has assumed logical meaning for it. The simultaneous use of the three definitions of meaning is very expedient for the discussion of physical theories and their comparison under the viewpoint of empirical truth. We may say, however, that for the actual system of physics, physical meaning is generally assumed.[16]

(7) The conditions 1-6 together with condition 8, which I shall deal with presently, lay down conditions of meaningfulness and thus specify what I have called the first principle of the verifiability theory of meaning. The second principle, which defines sameness of meaning, must now be added.

Although equivalence was not assumed to hold between direct and indirect sentences, there can be equivalences between *indirect sentences; and it is indispensable for a theory of meaning* to lay down the condition that such equivalences lead to sameness of meaning. The three forms of possibility lead to a corresponding division for this category. If the equivalence is analytic, i.e. follows from the rules of logic, we have logical identity of meaning; if the equivalence is synthetic and follows from the laws of physics, we have physical identity of meaning. Technical identity of meaning is a dispensable concept. The above example of Einstein's principle of equivalence illustrates physical identity of meaning. It is an important part of physical research to discover such identities; this has been very well pointed out by Feigl,[17] who mentions the identity of light waves and electromagnetic waves, of heat and average kinetic energy of the molecules, etc., and discusses in this frame the mind-body problem.

As before, it can be useful to employ different definitions of identity of meaning in the same context; for instance, to use Frege's example, we may say that the expressions "the morning star" and "the evening star" have the same physical meaning, but not the same logical meaning. Furthermore, it appears necessary to subdivide the category of logical identity of meaning and to define synonymity by a much narrower requirement, so that, for instance, the terms "five" and "number of regular polyhedrons" are not synonymous, though they can be shown to be tautologically equivalent. A definition of synonymity of this kind was recently given in a highly satisfactory form by Carnap.[18]

It is the practical significance of the second principle of the verifiability theory of meaning that it allows for the translation of expressions of emotive languages into an empirical language.

Statements about mystical visions, or about "moral truth", can thus be given an empirical content, though such a translation may provoke violent opposition on the side of the advocate of superempirical meanings. Visions then acquire the status of dreams, or hallucinations, of certain persons; and "moral truths", the status of beliefs of certain persons. I refer to another publication.[19]

This discussion of identity of meaning answers the objections raised by Bertrand Russell,[20] which are based on earlier versions of the verifiability theory of meaning. He argues that, for the positivist, speaking about other persons' minds would mean the same as speaking about dreamed objects. I doubt whether this is true even for the earliest form of positivism, where the distinction between dream and waking state was already made. And it certainly is not true for a verifiability theory of meaning which allows for indirect verification, since in such a theory identity of meaning does not hold between indirect sentences and observation sentences.

(8) I now come to the discussion of a set of rules which had been overlooked in the earlier discussion of meaning and which were first introduced in my analysis of the problem of unobservables of quantum mechanics.

Going from the sentences of the basis *O* to indirect sentences is the same as going from observables to unobservables. We are all agreed that this inference is not a matter of belief in a transcendental reality; in fact, if the verifiability theory of meaning is accepted, this would be a belief in something meaningless. So, it must be a form of inductive inference; and thus arises the difficulty of how to make inductions which connect unobservables to observables. Inductions concerning a certain object must start with some properties of this object and then may add further properties to it. But since we have no knowledge of the unobservables, we have nothing to start with. We cannot say: so far the things have existed when we were not looking at them, therefore they will do the same in the future. The premise of this inference is not verified by any observation, and cannot be so verified because of the definition of the term "unobserved object".[21]

This simple analysis shows that statements about unobservables cannot be introduced unless certain conventions are added to our language. In my investigation of quantum mechanics,[22] I have shown that the usual language of science includes the convention that unobservables follow the same physical laws as observables;

in particular, that they satisfy the principle of causality, which for observables is an empirical law. Without such a convention, the language of unobservables is incomplete and not accessible to verification. It leads to statements of the same kind as, for instance, the statement that during the night all things, including our bodies, have become ten times larger. Such paradoxes spring from incompleteness of language, and are easily eliminated by making the language complete through suitable rules.

I will call rules of this kind *extension rules.* They extend the range of laws from observables to unobservables. They are conventions determining the structure of language. Varying the convention, we arrive at a set of *equivalent descriptions,* which are all true to the same extent. Among them, the description for which unobservables follow the same laws as observables is called the *normal system.* Yet the necessity of conventions opens up an empirical investigation: it requires an analysis whether a certain convention can be carried through, i.e., does not lead to contradictory statements about observables. Classical physics can be carried through consistently on the convention that the laws of unobservables are the same as the law of observables. Classical physics, therefore, has a normal system—which is nothing but the usual realistic language. Quantum physics does not admit of a normal system. This is a domain where the mentioned extension rule breaks down and other extension rules have to be used. We have therefore to distinguish between admissible and inadmissible conventions of language. Whether a convention is admissible, is an empirical question. If it is admissible, its use makes the resulting language not "more true" than the use of another admissible convention.

I hope these formulations will clarify a controversy which has arisen about the exposition of realism in my book "Experience and Prediction". I have introduced the term *illata* for objects that can be described only in indirect sentences.[23] This term is as legitimate as the term "indirect sentence". Certainly, illata are not determined unless the extension rules of the language are given; but this fact does not make them "less real". They share this property with all other things, since nothing can be described adequately in an incomplete language. Furthermore, in the discussion of a cubical world[24] in which bird-like shadows are visible, I have argued for a realistic language which speaks about the birds although they are unobservable and thus are illata. In the terminology of my analysis of quantum mechanics, I would say that in this world the realistic language constitutes the normal system, whereas the positivistic language, which speaks only about the shadows on the walls, is a restrictive interpretation. My argument therefore is that there is no reason to introduce a restrictive interpretation if a normal system exists.[25] The same would apply to quantum mechanics if, for instance, the corpuscle interpretation could be carried through without causal anomalies, even if Heisenberg's indeterminacy were to remain valid and a simultaneous ascertainment of position and momentum were impossible.

When I say that we have inductive evidence for the existence of the external world, I mean the fact that the realistic language can be carried through for the macrocosm, that there is a normal system. This is a verifiable and meaningful statement; but of course, it is so only after the language is made complete through extension rules. That the realistic hypothesis is empirical, and not a faith, or perhaps a truism, is shown by the strange results of quantum mechanics, which exclude the realistic hypothesis, in the usual sense, for the microcosm. Here we can speak about physical reality only in an indirect way, using the duality of wave and particle description; instead of the normal system, we use a set of equivalent descriptions, none of which has the directness of the normal system. A realistic language in the sense of the language of the macrocosm is here impossible; if there is any realism left, it is certainly not a "naive realism".

I should like to add the remark that the reality problem of quantum mechanics has been construed as resulting from the influence of the human observer and thus as confirming idealistic philosophies, according to which the *ego* creates the external world; in another version of such ideas, it has been argued that the line of demarcation between observer and external object cannot be clearly drawn. I do not think such a "metaphysical" interpretation of quantum mechanics is tenable. The observables of quantum mechanics are not human perceptions, but physical occurrences of a certain kind; and the quantum mechanical difficulties begin when the realm of these observables, or phenomena, is to be supplemented by a realm of unobservables, or interphenomena. It is therefore not the step from the human observer to the external object, but the step from macrophysics to microphysics which involves the difficulties. The quantum-mechanical indeterminacy is an entirely physical affair; i.e., it expresses a structural property of the physical world, which has nothing to do with the fact that this world is observed by human beings.[26] That the indeterminacy influences our system of knowledge has other reasons. Our bodies are macroscopic organisms, and our direct observations are restricted to macroscopic objects. Our knowledge of the microcosm is acquired by way of the macrocosm, and on this transition the indeterminacy enters our statements about the microcosm.

In order to construct an adequate realistic language for quantum phenomena I have suggested to use a three-valued logic, which possesses a category of indeterminate sentences between those of true and of false sentences. Since the indeterminate sentences are neither true nor false, the objection has been raised that my interpretation of quantum mechanics contradicts the verifiability criterion of meaning.[27] This is a misunderstanding. The theory of meaning has been emancipated, for a long while, from its first dogmatic version and has assumed a moderate version, which admits of modified forms of verification. In order to include the three-valued logic of quantum mechanics, the conditions of meaningfulness, in particular, the extension rules, have to be widened so as to admit the category of indeterminate statements. Since these statements are connected by well-defined rules with the observational basis of language, they are as legitimate as any other statements exempt from direct verification. To put it briefly: a quantum-mechanical statement is meaningful if it is verifiable (in the moderate sense of probability verification) as true, false, or indeterminate.

III.

This is the state of the verifiability theory of meaning in 1950. From 1923 to 1950 there has been a development abundant in results. It sprang from the cooperation of men, and of groups of men. The Vienna circle and the Berlin group, in spite of all their differences, remained friends and worked together; and their work was continued in the wider group of logical empiricism, which grew out of their alliance and includes men from many countries, among them many from this country and many who have made this country their second home. The progressive development of their views is reflected in the development of their theory of meaning. What was guesswork and program in the beginning, has been subject to methods of logical analysis and has acquired the status of an elaborate logical theory. It could go through this development only because the earlier versions of this theory were formulated and defended with the ardor and stubbornness of the philosopher who thinks to have found the truth; and because a better truth is strong enough to overcome the stubbornness of the philosopher and to kindle a new ardor for better formulation. Let us be aware that, if not everything, yet philosophy is in flux; and that those who discover new results learn by their mistakes. The method of trial and error is not only the tool of the experimental scientist; it is also the tool of the philosopher.

NOTES

[1] R. Carnap, Der logische Aufbau der Welt, Berlin, 1928.

[2] "Ziele und Wege der physikalischen Erkenntnis", in H. Geiger and K. Scheel, *Handbuch der Physik,* Vol. IV, 1929, pp. 1-80. See pp. 16-24.

[3] Experience and Prediction, Chicago 1938, p. 111, p. 130.

[4] Wahrscheinlichkeitslehre, Leiden 1935.

[5] Logistic Empiricism in Germany, *Journ. of Philosophy,* vol. 33, 1936, pp. 147-150, p. 154.

[6] R. Carnap, Testability and Meaning, *Philosophy of Science,* vol. 4, 1936, p. 419, and vol. 5, 1937, p. 1.

[7] H. Feigl, Existential Hypotheses, *Philosophy of Science,* vol. 17, 1950, p. 35; The Mind-Body Problem, *Revue Internationale de Philosophie,* No. 11, 1950, p. 64. The same issue of the *Revue* contains interesting articles by Carnap and Hempel.

[8] I think these remarks correspond to views about the verifiability theory of meaning recently presented by P. Marhenke, Presidential Address to the Pacific Division of the American Philosophical Association, Christmas 1949, to be published in *Philosophical Review.*

[9] See the discussion of such sentences in my book "Experience and Prediction", Chicago 1938, pp. 133-135.

[10] Bertrand Russell, Logical Positivism, *Revue Internationale de Philosophie,* No. 11, 1950, p. 19.

[11] R. Carnap, On Inductive Logic, *Philosophy of Science,* vol. 12, 1945, p. 95.

[12] I would therefore say that for a fallacious argument, as a whole, there is no rational reconstruction. Such argument can be analyzed as follows: the argument contains a "jump" from one point P_1 to another point P_2. From the premises up to P_1, and from P_2 to the conclusion, we can give a rational reconstruction; but for the transition from P_1 to P_2, there is no rational reconstruction. It seems to me that even the term "reconstruction"

cannot be defined without some criterion of validity, and that fallacious thinking cannot be reconstructed unless the reconstruction is rational at least with respect to parts of the argument.

[13] Carnap's paper, "Probability as a Guide in Life", *Journ. of Philos.*, vol. 44, 1947, p. 141, does not contain any proof of this kind.

[14] I refer to the discussion in my book "The Theory of Probability", Berkeley 1949, §71 and §88.

[15] Elements of Symbolic Logic, New York 1947, chap. VIII.

[16] It is a pity, therefore, that Carnap in his enlightening book "Meaning and Necessity", Chicago 1947, does not speak at all about physical meaning and physical possibility.

[17] H. Feigl, The Mind-Body Problem, *Revue Internationale de Philosophie*, No. 11, 1950, p. 80.

[18] R. Carnap, Meaning and Necessity, Chicago 1947, §§14-15. Incidentally, it seems to me that the outcome of this book can be summarized in the following thesis: the major point is the definition of the terms "having the same extension" and "having the same intension"; interpretations of the terms "extension" and "intension" can then be added more or less arbitrarily.

[19] Experience and Prediction, Chicago 1938, pp. 66-68.

[20] Bertrand Russell, Human Knowledge, New York 1948, p. 449.

[21] This argument was constructed by W. T. Stace, Refutation of Realism, *Mind* 43, 1934. It is also discussed in my book "Philosophic Foundations of Quantum Mechanics," Berkeley 1944, p. 18. It is shown there that the solution consists, not in a refutation of realism, but in a reformulation of realism.

[22] See p. 19 of my book mentioned in the preceding footnote.

[23] Experience and Prediction, Chicago 1938, p. 212.

[24] Ibid. §14.

[25] This answers certain objections by H. Feigl, Existential Hypotheses, *Philos. of Science*, vol. 17, 1950, p. 54, who unfortunately overlooks the exposition of the problem in my book "Philosophic Foundations of Quantum Mechanics" and bases his critique merely on my book "Experience and Prediction".

[26] See my book, "Philosophic Foundations of Quantum Mechanics", p. 15. An imaginary macroscopic analogy of quantum mechanics is described there on pp. 38-39.

[27] E. Nagel, *Journ. of Philosophy*, vol. 42, 1945, pp. 437-444. See also my reply, vol. 43, 1946, p. 244.

THE CONCEPT OF COGNITIVE SIGNIFICANCE: A RECONSIDERATION

CARL G. HEMPEL
Yale University

1. THE GENERAL EMPIRICIST CONCEPTION OF COGNITIVE AND OF EMPIRICAL SIGNIFICANCE

It is a basic principle of contemporary empiricism that a sentence makes a cognitively significant assertion, and thus can be said to be either true or false, if and only if either (1) it is analytic or contradictory—in which case it is said to have purely logical meaning or significance—or else (2) it is capable, at least potentially, of test by experiential evidence—in which case it is said to have empirical meaning or significance. The general epistemological orientation expressed in this principle, and especially in its second part, the so-called testability criterion of empirical meaning, is not peculiar to empiricism alone: it is characteristic also of contemporary operationism, and in a sense of pragmatism as well; for the pragmatist maxim that a difference must make a difference to be a difference may well be construed as insisting that a verbal difference between two sentences must make a difference in experiential implications if it is to reflect a difference in meaning.

How this general conception of cognitively significant discourse led to the rejection, as devoid of logical and empirical meaning, of various formulations in speculative metaphysics, and even of certain hypotheses offered within empirical science, is too well known to require recounting. I think that the general intent of the empiricist criterion of meaning is basically sound, and that notwithstanding much oversimplification in its use, its critical application has been, on the whole, enlightening and salutary. I feel less confident, however, about the possibility of restating the general idea in the form of precise and general criteria which establish sharp dividing lines (a) between statements of purely logical and statements of empirical significance, and (b) between those sentences which do have cognitive significance and those which do not.

In the present paper, I propose to reconsider these distinctions as conceived in recent empiricism, and to point out some of the difficulties they seem to present. The discussion will concern mainly the second of the two distinctions; in regard to the first, I shall have to limit myself to a few brief remarks.

2. THE EARLIER "TESTABILITY" CRITERIA OF MEANING AND THEIR SHORTCOMINGS

Let us note first that any general criterion of cognitive significance will have to meet certain requirements if it is to be at all acceptable. Of these, we note one, which we shall consider here as expressing a necessary, though by no means sufficient, "*condition of adequacy*" for criteria of cognitive significance.

(A) If under a given criterion of cognitive significance, a sentence N is non-significant, then so must be all truth-functional compound sentences in which N occurs non-vacuously as a component. For if N cannot be significantly assigned a truth value, then it is impossible to assign truth values to the compound sentences containing N; hence, they should be qualified as non-significant as well.

We note two corollaries of requirement (A):

(A1) If under a given criterion of cognitive significance, a sentence S is non-significant, then so must be its denial, $\sim S$.

(A2) If under a given criterion of cognitive significance, a sentence N is non-significant, then so must be any conjunction $N.S$ and any alternation $N \mathrm{v} S$, no matter whether S is significant under the given criterion or not.

We now turn to the initial attempts made in recent empiricism to establish general criteria of cognitive significance. Those attempts were governed by the consideration that a sentence, to make an empirical assertion, must be capable of being borne out by, or conflicting with, phenomena which are potentially capable of being directly observed. Sentences describing such potentially observable phenomena—no matter whether the latter do actually occur or not—may be called observation sentences. More specifically, an *observation sentence* might be construed as a sentence—no matter whether true or false—which ascribes to some specified object, or group of objects, of macroscopic size, some particular *observable characteristic,* i.e., a characteristic whose presence or absence can, under favorable circumstances, be ascertained by direct observation.

The task of setting up criteria of empirical significance is thus transformed into the problem of characterizing in a precise manner the relationship which obtains between a hypothesis and one or more observation sentences whenever the phenomena described by the latter either confirm or disconfirm the hypothesis in question. The ability of a given sentence to enter into that relationship to some set of observation sentences would then characterize its testability-in-principle, and thus its empirical significance. Let us now briefly examine the major attempts that have been made to obtain criteria of significance in this manner.[1]

(2.1) One of the earliest criteria is expressed in the so-called *verifiability requirement*. According to it, a sentence is empirically significant if and only if it is non-analytic and is capable of complete verification by, i.e., logical deduction from, some finite logically consistent set of observation sentences. These observation sentences need not be true, for what the criterion is to explicate is testability by "potentially observable phenomena", or testability "in principle".

In accordance with the general conception of cognitive significance outlined earlier, a sentence will now be classified as cognitively significant if either it is analytic or contradictory, or it satisfies the verifiability requirement.

The criterion thus obtained, however, is unacceptable for several reasons, of which we mention here the following:

a. Let S be a sentence which does, and N a sentence which does not satisfy the verifiability requirement. Then S is deducible from some set of observation sentences; hence, by a familiar rule of logic, $S \mathrm{v} N$ is deducible from the same set, and therefore cognitively significant according to our criterion. This violates condition (A2) above.

b. Let us assume that the properties of being a stork and of being red-legged are both observable characteristics, and that the former does not logically entail the latter. Then the sentence

($S1$) All storks are red-legged

is neither analytic nor contradictory; and clearly, it is not deducible from a finite set of observation sentences. Hence, under the contemplated criterion, $S1$ is devoid of empirical significance; and so are all other sentences purporting to express universal regularities or general laws. This is in itself a highly objectionable consequence of our criterion. But furthermore, it is readily seen that the denial of $S1$

($\sim S1$) There exists at least one stork that is not red-legged

is deducible from any two observation sentences of the type 'a is a stork' and 'a is not red-legged'. Hence, $\sim S1$ is cognitively significant under our criterion, and this constitutes a violation of condition (A1).

(2.2) A second criterion, the so-called *falsifiability requirement,* qualifies a sentence S as significant if S is not contradictory and is capable of complete falsification (refutation) by some consistent finite set of observation sentences.

But this requirement is tantamount to the condition that $\sim S$ satisfy the verifiability requirement; therefore, the new criterion, too, violates (A1) and (A2).

(2.3) A more complex criterion has been proposed by Ayer;[2] it is rather involved, and we shall not state it here, for it can be shown that it violates (A2) by qualifying as significant any alternation with at least one significant component. Furthermore, as has been shown by Church (1949), Ayer's criterion has the unacceptable consequence that if there are three observation sentences none of which entails either of the others, then, if S is any sentence whatever, either S or $\sim S$ is significant under the criterion.

(2.4) An alternative criterion recently suggested by O'Connor (1950) as a revision of Ayer's formulation is subject to a slight variant of Church's stricture: It can be shown that if there are three observation sentences none of which entails any of the others, and if S is any non-compound sentence, then either S or $\sim S$ is significant under O'Connor's criterion.

All the criteria considered so far attempt to explicate the concept of empirical significance by specifying certain logical connections which must obtain between a significant sentence and suitable observation sentences. It seems now that this type of approach offers little hope for the attainment of precise criteria of meaningfulness: this conclusion is suggested by the preceding survey of some representative attempts, and it receives additional support from certain further considerations, some of which will be presented in the following sections.

3. Characterization of Significant Sentences by Criteria for Their Constituent Terms

An alternative procedure suggests itself which again seems to reflect well the general viewpoint of empiricism: It might be possible to characterize cognitively significant sentences by certain conditions which their constituent terms have to satisfy.

Specifically, it would seem reasonable to say that all extralogical terms[3] in a significant sentence must have experiential reference, and that therefore their meanings must be capable of explication by reference to observables exclusively.[4] In order to exhibit certain analogies between this approach and the previous one, we adopt the following terminological conventions:

Any term that may occur in a cognitively significant sentence will be called a *cognitively significant term.* Furthermore, we shall understand by an *observation term* any term which either (a) is an *observation predicate,* i.e., signifies some observable characteristic (as do the terms 'blue', 'warm', 'soft', 'coincident with', 'of greater apparent brightness than'), or (b) names some physical object of macroscopic size (as do the terms 'the needle of this instrument', 'the Moon', 'Krakatao Volcano', 'Greenwich, England', 'Julius Caesar').

Now while the testability criteria of meaning aimed at characterizing the cognitively significant sentences by means of certain inferential connections in which they must stand to some observation sentences, the alternative approach under consideration would instead try to specify the vocabulary that may be used in forming significant sentences. This vocabulary, the class of significant terms, would be characterized by the condition that each of its elements is either a logical term or else a term with empirical significance; in the latter case, it has to stand in certain definitional or explicative connections to some observation terms. This approach certainly avoids any violations of our earlier conditions of adequacy. Thus, e.g., if S is a significant sentence, i.e., contains cognitively significant terms only, then so is its denial, since the denial sign, and its verbal equivalents, belong to the vocabulary of logic and are thus significant. Again, if N is a sentence containing a non-significant term, then so is any compound sentence which contains N.

But this is not sufficient, of course. Rather, we shall now have to consider a crucial question analogous to that raised by the previous approach: Precisely how are the logical connections between empirically significant terms and observation terms to be construed if an adequate criterion of cognitive significance is to result? Let us consider some possibilities.

(3.1) The simplest criterion that suggests itself might be called the *requirement of definability.* It would demand that any term with empirical significance must be explicitly definable by means of observation terms.

This criterion would seem to accord well with the maxim of operationism that all significant terms of empirical science must be introduced by operational definitions. However, the requirement of definability is vastly too restrictive to be satisfactory, for many important terms of scientific and even pre-scientific discourse cannot be explicitly defined by means of observation terms.

In fact, as Carnap[5] has pointed out, an attempt to provide explicit definitions in terms of observables encounters serious difficulties as soon as disposition terms, such as 'soluble', 'malleable', 'electric conductor', etc., have to be accounted for; and many of these occur even on the pre-scientific level of discourse. These difficulties have been expounded so often[6] that we shall not restate them here, except for saying that they seem to be capable of resolution only by means of a suitable theory of modal connectives or by the use of methods other than explicit definition for the introduction of certain empirically significant terms. A fruitful method of the latter kind has been developed by Carnap.[7] It consists of introducing new terms by means of so-called *reduction sentences;* these are partial, or conditional, definitions of a certain type which includes explicit definition as a special subclass. Reduction sentences provide a satisfactory interpretation of the experiential import of a large class of disposition terms and permit a more adequate formulation of so-called operational definitions, which, in general, are not complete definitions at all. These successes suggested a greatly liberalized alternative to the requirement of definability:

(3.2) *The requirement of reducibility.* Every term with empirical significance must be capable of introduction, on the basis of observation terms, through chains of reduction sentences.

This requirement is characteristic of the liberalized versions of positivism and physicalism which, since about 1936, have superseded the older, overly narrow conception of a full definability of all terms of empirical science by means of observables,[8] and it avoids many of the shortcomings of the latter. Yet, reduction sentences do not seem to offer an adequate means for the introduction of the central terms of advanced scientific theories, often referred to as theoretical constructs. This is indicated by the following considerations: A chain of reduction sentences provides a necessary and a sufficient condition for the applicability of the term it introduces. (When the two conditions coincide, the chain is tantamount to an explicit definition.) But now take, for example, the concept of length as used in classical physical theory. Here, the length in centimeters of the distance between two points may assume any positive real number as its value; yet it is clearly impossible to formulate, by means of observation terms, a sufficient condition for the applicability of such expressions as 'having a length of $\sqrt{2}$ cm' and 'having a length of $\sqrt{2} + 10^{-100}$ cm'; for such conditions would provide a possibility for discriminating, in observational terms, between two lengths which differ by only 10^{-100} cm.

It would be ill-advised to argue that for this reason, we ought to permit only such values of the magnitude, length, as permit the statement of sufficient conditions in terms of observables. For this would rule out, among others,[9] all irrational numbers and would prevent us from assigning, to the diagonal of a square with sides of length 1, the length $\sqrt{2}$, which is required by Euclidean geometry. Hence, the principles of Euclidean geometry would not be universally applicable in physics. Similarly, the principles of the calculus would become inapplicable, and the system of scientific theory as we know it today would be reduced to a clumsy, unmanageable torso. This, then, is no way of meeting the difficulty. Rather, we shall have to analyze more closely the function of constructs in scientific theories, with a view to obtaining through such an analysis a more adequate characterization of cognitively significant terms.

Theoretical constructs occur in the formulation of scientific theories. These may be conceived of, in their advanced stages, as being stated in the form of deductively developed axiomatized systems. Classical mechanics, or Euclidean or some Non-Euclidean form of geometry in physical interpretation, present examples of such systems. The extra-logical terms used in a theory of this kind may be divided, in familiar manner, into primitive or basic terms, which are not defined within the theory, and defined terms, which are explicitly defined by means of the primitives. Thus, e.g., in Hilbert's axiomatization of Euclidean geometry,[9a] the terms 'point', 'straight line', 'between', are among the primitives, while 'line segment', 'angle', 'triangle', 'length' are among the defined terms. The basic and the defined terms together with the terms of logic constitute the vocabulary out of which all the sentences of the theory are constructed. The latter are divided, in an axiomatic presentation, into primitive statements (also called postulates or basic statements) which, in the theory, are not derived from any other statements, and derived ones, which are obtained by logical deduction from the primitive statements.

From its primitive terms and sentences, an axiomatized theory can be developed by means of purely formal principles of definition and deduction, without any consideration of the empirical significance of its extra-logical terms. Indeed, this is the standard procedure employed in the axiomatic development of uninterpreted mathematical theories such as those of abstract groups or rings or lattices, or any form of pure (i.e., non-interpreted) geometry.

However, a deductively developed system of this sort can constitute a scientific theory only if it has received an empirical interpretation[10] which renders it relevant to the phenomena of our experience. Such interpretation is given by assigning a meaning, in terms of observables, to certain terms or sentences of the formalized theory. Frequently, an interpretation is given not for the primitive terms or statements of the theory, but rather for some of the terms definable by means of the primitives, or for some of the sentences deducible from the postulates.[11] Furthermore, interpretation may amount to only a partial assignment of meaning. Thus, e.g., the rules for the measurement of length by means of a standard rod may be considered as providing a *partial* empirical interpretation for the term 'the length, in centimeters, of interval *i*', or alternatively, for some sentences of the form 'the length of interval *i* is *r* centimeters'. For indeed, the method is applicable only to intervals of a certain medium size, and even for the latter it does not constitute a full interpretation — for at least two different reasons: First, as was noted before, no sentence expressible in observation terms and logical terms alone can state a sufficient condition (let alone a necessary and sufficient one) for a sentence of the form 'the length of interval *i* is *r* centimeters'; and secondly, the use of a standard rod does not constitute the only way of determining length. Various alternative procedures are available involving the measurement of other magnitudes which are connected, by general laws, with the length that is to be determined.

This last observation, concerning the possibility of an indirect measurement of length by virtue of certain laws, suggests an important reminder: It is not correct to speak, as it is often done, of "the experiential meaning" of a term or a sentence in isolation. In the language of science, and for similar reasons even in pre-scientific discourse, a single statement usually has no experiential implications. A single sentence in a scientific theory, for example, does not, as a rule, entail any observation sentences; consequences asserting the occurrence of certain observable phenomena can be derived from it only by conjoining it with a set of other, subsidiary, hypotheses. Of the latter, some will usually be observation sentences, others will be previously accepted theoretical statements. Thus, e.g., the relativistic theory of the deflection of light rays in the gravitational field of the sun entails assertions about observable phenomena only if it is conjoined with a considerable body of astronomical and optical theory as well as a large number of specific statements about the instruments used in those observations of solar eclipses which serve to test the hypothesis in question.

Hence, the phrase, 'the empirical meaning of expression *E*' is elliptical in character: What a given expression "means" in regard to potential empirical data is relative to two factors, namely:

I. *the linguistic framework L* to which the expression belongs. Its rules determine, in particular, what sentences — observational or otherwise — may be inferred from a given statement or class of statements;

II. the theoretical context in which the expression occurs, i.e., the class of those statements in *L* which are available as subsidiary hypotheses.

Thus, to mention just one more example, the sentence formulating Newton's law of gravitation has no experiential meaning by itself; but when used in a language whose logical apparatus permits the development of the calculus, and when combined with a suitable system of other hypotheses — including sentences which connect some of the theoretical terms with observation terms and thus establish a partial interpretation — then it has a bearing on observable phenomena in a large variety of fields. — Analogous considerations are applicable to the term 'gravitational field', for example. It can be considered as having experiential meaning only within the context of a theory, which must be at least partially interpreted; and the experiential meaning of the term — as expressed, say, in the form of operational criteria for its application — will depend again on the theoretical system at hand, and on the logical characteristics of the language within which it is formulated.

4. Cognitive Significance as a Characteristic of Interpreted Systems

The preceding considerations point to the conclusion that a satisfactory criterion of cognitive significance cannot be reached through the second avenue of approach here considered, namely by means of specific requirements for the terms which make up significant sentences. This result accords with a general characteristic of scientific (and, in principle, even pre-scientific) theorizing: Theory formation and concept formation go hand in hand; neither can be carried on successfully in isolation from the other.

If, therefore, cognitive significance can be attributed to anything, then only to entire theoretical systems formulated in a language with a well-determined structure. And the decisive mark of cognitive significance in such a system appears to be the existence of an interpretation for it in terms of observables. Such an interpretation might be formulated, for example, by means of conditional or biconditional sentences connecting non-observational terms of the system with observation terms in the given language; the latter as well as the connecting sentences may or may not belong to the theoretical system.

But the requirement of partial interpretation is extremely liberal; it is satisfied, for example, by the system consisting of contemporary physical theory combined with some set of principles of speculative metaphysics, even if the latter have no empirical interpretation at all. Within the total system, these metaphysical principles play the rôle of what K. Reach and also O. Neurath liked to call *isolated sentences:* They are neither purely formal truths or falsehoods, demonstrable or refutable by means of the logical rules of the given language system; nor do they have any experiential bearing; i.e., their omission from the theoretical system would have no effect on its explanatory and predictive power in regard to potentially observable phenomena (i.e., the kind of phenomena described by observation sentences).[12] Should we not, therefore, require that a cognitively significant system contain no isolated sentences? The following criterion suggests itself:

(4.1) A theoretical system is cognitively significant if and only if it is partially interpreted to at least such an extent that none of its primitive sentences is isolated.

But this requirement may bar from a theoretical system certain sentences which might well be viewed as permissible and indeed desirable. By way of a simple illustration, let us assume that our theoretical system T contains the primitive sentence

$$(S1) \qquad (x)\big(P_1x \supset (Qx \equiv P_2x)\big)$$

where 'P_1' and 'P_2' are observation predicates in the given language L, while 'Q' functions in T somewhat in the manner of a theoretical construct: it occurs in only one primitive sentence of T, namely $S1$. Now $S1$ is not a truth or falsehood of formal logic; and furthermore, if $S1$ is omitted from the set of primitive sentences of T, then the resulting system, T', possesses exactly the same systematic, i.e., explanatory and predictive, power as T. Our contemplated criterion would therefore qualify $S1$ as an isolated sentence which has to be eliminated — excised by means of Occam's razor, as it were — if the theoretical system at hand is to be cognitively significant.

But this may be an overly strict procedure; indeed, it is possible to take a much more favorable view of $S1$ by treating it as a partial definition for the theoretical term 'Q'. Thus conceived, $S1$ specifies that in all cases where the observable characteristic P_1 is present, 'Q' is applicable if and only if the observable characteristic P_2 is present as well. In fact, $S1$ is an instance of those partial, or conditional, definitions which Carnap calls bilateral reduction sentences. These sentences are explicitly qualified by Carnap as analytic (though not, of course, as truths of formal logic), essentially on the ground that all their consequences which are expressible by means of observation predicates (and logical terms) alone are truths of formal logic.[13]

Let us pursue this line of thought a little further. This will lead us to some observations on analytic sentences and then back to the question of the adequacy of (4.1).

Suppose that we add to our system T the further sentence

$$(S2) \qquad (x)\big(P_3x \supset (Qx \equiv P_4x)\big)$$

where 'P_3', 'P_4' are additional observation predicates. Then, on the view that "every bilateral reduction sentence is analytic",[14] $S2$ would be analytic as well as $S1$. Yet, the two sentences taken jointly entail non-analytic consequences which are expressible in terms of observation predicates alone, such as[15]

(O)

$$(x)\big(\sim(P_1x \cdot P_2x \cdot P_3x \cdot \sim P_4x) \cdot \sim(P_1x \cdot \sim P_2x \cdot P_3x \cdot P_4x)\big)$$

But one would hardly want to admit the consequence that the conjunction of two analytic sentences may be synthetic. Hence if the concept of analyticity can be applied at all to the sentences of interpreted deductive systems, then it will have to be relativized with respect to the theoretical context at hand. Thus, e.g., *S*1 might be qualified as analytic relatively to the system *T*, whose remaining postulates do not contain the term '*Q*', but as synthetic relatively to the system *T* enriched by *S*2. — Strictly speaking, the concept of analyticity has to be relativized also in regard to the rules of the language at hand, for the latter determine what observational or other consequences are entailed by a given sentence. This need for at least a twofold relativization of the concept of analyticity was almost to be expected in view of those considerations which required the same twofold relativization for the concept of experiential meaning of a sentence.

If, on the other hand, we decide not to permit *S*1 in the rôle of a partial definition and instead reject it as an isolated sentence, then we are led to an analogous conclusion: Whether a sentence is isolated or not is relative to the linguistic frame and to the theoretical context at hand: While *S*1 is isolated relatively to *T* (and the language in which both are formulated), it acquires definite experiential implications when *T* is enlarged by *S*2.

Thus we find, on the level of interpreted theoretical systems, a peculiar rapprochement, and partial fusion, of some of the problems pertaining to the concepts of cognitive significance and of analyticity: Both concepts need to be relativized; and a large class of sentences may be viewed, apparently with equal right, as analytic in a given context, or as isolated, or non-significant, in respect to it.

In addition to barring, as isolated in a given context, certain sentences which could just as well be construed as partial definitions, the criterion (4.1) has another serious defect: Of two logically equivalent formulations of a theoretical system it may qualify one as significant while barring the other as containing an isolated sentence among its primitives. For assume that a certain theoretical system *T*1 contains, among its primitive sentences *S*′, *S*″, . . . exactly one, *S*′, which is isolated. Then *T*1 is not significant under (4.1). But now consider the theoretical system *T*2 obtained from *T*1 by replacing the two first primitive sentences, *S*′, *S*″, by one, namely their conjunction. Then, under our assumptions, none of the primitive sentences of *T*2 is isolated, and *T*2, though equivalent to *T*1, is qualified as significant by

(4.1). In order to do justice to the intent of (4.1), we would therefore have to lay down the following stricter requirement:

(4.2) A theoretical system is cognitively significant if, and only if, it is partially interpreted to such an extent that in no system equivalent to it at least one primitive sentence is isolated.

Let us apply this requirement to some theoretical system whose postulates include the two sentences *S*1 and *S*2 considered before, and whose other postulates do not contain '*Q*' at all. Since the sentences *S*1 and *S*2 together entail the sentence *O*, the set consisting of *S*1 and *S*2 is logically equivalent to the set consisting of *S*1, *S*2, and *O*. Hence, if we replace the former set by the latter, we obtain a theoretical system equivalent to the given one. In this new system, both *S*1 and *S*2 are isolated since, as can be shown, their removal does not affect the explanatory and predictive power of the system in reference to observable phenomena. To put it intuitively, the systematic power of *S*1 and *S*2 is the same as that of *O*. Hence, the original system is disqualified by (4.2). From the viewpoint of a strictly sensationalist positivism as perhaps envisaged by Mach, this result might be hailed as a sound repudiation of theories making reference to fictitious entities, and as a strict insistence on theories couched exclusively in term of observables. But from a contemporary vantage point, we shall have to say that such a procedure overlooks or misjudges the important function of constructs in scientific theory: The history of scientific endeavor shows with increasing clarity that if we wish to arrive at precise, comprehensive, and well-confirmed general laws, we have to rise above the level of direct observation. The phenomena directly accessible to our experience are not connected by general laws of great scope and rigor. Theoretical constructs are needed for the formulation of such higher-level laws. One of the most important functions of a well-chosen construct is its potential ability to serve as a constituent in ever new general connections that may be discovered; and to such connections we would blind ourselves if we insisted on banning from scientific theories all those terms and sentences which could be "dispensed with" in the sense indicated in (4.2). In following such a narrowly phenomenalistic or positivistic course, we would deprive ourselves of the tremendous fertility of theoretical constructs, and we would also often render the formal structure of the expurgated theory clumsy and inefficient.

Criterion (4.2), then, must be abandoned, and considerations such as those outlined in this paper seem to lend strong support to the conjecture that no adequate alternative to it can be found; i.e., that it is not possible to formulate general and precise criteria which would separate those partially interpreted systems whose isolated sentences might be said to have a significant function from those in which the isolated sentences are, so to speak, mere useless appendages.

We concluded earlier that cognitive significance in the sense intended by recent empiricism and operationism can at best be attributed to sentences forming a theoretical system, and perhaps rather to such systems as wholes. Now, rather than trying to replace (4.2) by some alternative, we will have to recognize further that cognitive significance in a system is a matter of degree: Significant systems range from those whose entire extra-logical vocabulary consists of observation terms, through theories whose formulation relies heavily on theoretical constructs, on to systems with hardly any bearing on potential empirical findings. Instead of dichotomizing this array into significant and non-significant systems, it would seem less arbitrary and more promising to appraise or compare different theoretical systems in regard to such characteristics as these:

a. the clarity and precision with which the theories are formulated, and with which the logical relationships of their elements to each other and to expressions couched in observational terms have been made explicit;
b. the systematic, i.e., explanatory and predictive, power of the systems in regard to observable phenomena;
c. the formal simplicity of the theoretical system with which a certain systematic power is attained;
d. the extent to which the theories have been confirmed by experiential evidence.

Many of the speculative philosophical approaches to cosmology, biology, or history, for example, would make a poor showing on practically all of these counts and would thus prove no matches to available rival theories, or would be recognized as so unpromising as not to warrant further study or development.

If the procedure here suggested is to be carried out in detail, so as to become applicable also in less obvious cases, then it will be necessary, of course, to develop general standards, and theories pertaining to them, for the appraisal and comparison of theoretical systems in the various respects just mentioned. To what extent this can be done with rigor and precision cannot well be judged in advance. In recent years, a considerable amount of work has been done towards a definition and theory of the concept of degree of confirmation, or logical probability, of a theoretical system,[16] and several contributions have been made towards the clarification of some of the other ideas referred to above.[17] The continuation of this research represents a challenge for further constructive work in the logical and methodological analysis of scientific knowledge.

NOTES

[1] For a more detailed survey, see Hempel (1950). The present study is intended to extend the analysis presented in that earlier article.

[2] Ayer (1946), p. 13. This criterion is suggested by Ayer as a correction of an earlier, inadequate one.

[3] An extra-logical term is one that does not belong to the specific vocabulary of logic. The following phrases, and those definable by means of them, are typical examples of logical terms: 'not', 'and', 'or', 'if . . . then', 'all', 'some', '. . . is an element of class - - -'. Whether it is possible to make a sharp theoretical distinction between logical and extra-logical terms is a controversial issue related to the problem of discriminating between analytic and synthetic sentences. For the purpose at hand, we may simply assume that the logical vocabulary is given by enumeration.

[4] For a detailed exposition and critical discussion of this idea, see H. Feigl's stimulating and enlightening article (1950).

[5] Cf. (1936/37), especially section 7.

[6] For a brief survey, see Hempel (1950), section 4.

[7] See especially (1936/37), also (1938), Part III.

[8] Cf. the decisive analysis in Carnap (1936/37), especially section 15; also see the briefer presentation of the liberalized point of view in Carnap (1938).—The two positions characterized by the definability and the reducibility criterion, respectively, are related also to the points of view which Feigl (1950) discusses under the headings ''Naive conventionalistic phenomenalism'' and ''Critical phenomenalism''.

[9] Strictly speaking, *no* real number value at all would be permitted by the contemplated rule.

[9a] Cf. Hilbert (1913).

[10] The interpretation of formal theories has been studied extensively by Reichenbach, especially in his pioneer analyses of space and time in classical and in relativistic physics. He describes such interpretation as the establishment of *coordinating definitions* (Zuordnungsdefinitionen) for certain terms of the formal theory. (See, for example, Reichenbach (1928)).—More recently, Northrop (cf. (1947), Ch. VII, and also the detailed study

of the use of deductively formulated theories in science, ibid., Chs. IV, V, VI) and H. Margenau (cf., for example, (1935)) have discussed certain aspects of this process under the title of *"epistemic correlation"*.

[11] A somewhat fuller account of this type of interpretation may be found in Carnap (1939), §24.—The articles by Spence (1944) and by MacCorquodale and Meehl (1948) provide enlightening illustrations of the use of theoretical constructs in a field outside that of the physical sciences, and of the difficulties encountered in an attempt to analyze in detail their function and interpretation.

[12] What is meant here by explanatory and predictive power in regard to observable phenomena is sufficiently clear for the purposes of the present investigation. A closer study of these concepts, and a proposal for an explicit definition of the concept of systematic, i.e., explanatory and predictive, power of a theoretical system has been presented by Hempel and Oppenheim (1948): Part IV.

[13] Cf. Carnap (1936/37), especially sections 8 and 10.

[14] Carnap (1936/37), p. 452.

[15] The sentence O is what Carnap calls the *representative sentence* of the couple consisting of the sentences $S1$ and $S2$; see (1936/37), pp. 450-453.

[16] Cf., for example, Carnap $(1945)_1$ and $(1945)_2$, and especially (1950). Also see Helmer and Oppenheim (1945) and, for a critique of these approaches, Reichenbach (1950).

[17] On simplicity, cf. especially Popper (1935), Ch. V; Reichenbach (1938), §42; Goodman $(1949)_1$, $(1949)_2$, (1950); on explanatory and predictive power, cf. Hempel and Oppenheim (1948).

BIBLIOGRAPHIC REFERENCES

Ayer, A. J. *Language, truth and logic.* 2nd ed., London, 1946.

Carnap, R. Testability and meaning. *Philosophy of science,* vol. 3 (1936) and 4 (1937).

Carnap, R. Logical foundations of the unity of science. In: *Internat. encyclopedia of unified science* I, 1. Univ. of Chicago Press, 1938.

Carnap, R. Foundations of logic and mathematics. *Internat. encyclopedia of unified science* I, 3. Univ. of Chicago Press, 1939.

Carnap, R. On inductive logic. *Philosophy of science,* vol. 12 (1945). (Referred to as $(1945)_1$ in this article)

Carnap, R. The two concepts of probability. *Philosophy and phenom. res.,* vol. 5 (1945). (Referred to as $(1945)_2$ in this article)

Carnap, R. Logical foundations of probability. Univ. of Chicago Press, 1950.

Church, A. Review of Ayer (1946). *The journal of symbolic logic,* vol. 14 (1949), pp. 52-53.

Feigl, H. Existential hypotheses. *Philosophy of science,* vol. 17 (1950).

Goodman, N. The logical simplicity of predicates. *The journal of symbolic logic,* vol. 14 (1949). (Referred to as $(1949)_1$ in this article)

Goodman, N. Some reflections on the theory of systems. *Philosophy and phenom. res.,* vol. 9 (1949). (Referred to as $(1949)_2$ in this article)

Goodman, N. An improvement in the theory of simplicity. *The journal of symbolic logic,* vol. 15 (1950).

Helmer, O. and Oppenheim, P. A syntactical definition of probability and of degree of confirmation. *The journal of symbolic logic,* vol. 10 (1945).

Hempel, C. G. and Oppenheim, P. Studies in the logic of explanation. *Philosophy of science,* vol. 15 (1948).

Hempel, C. G. Problems and changes in the empiricist criterion of meaning. *Revue internationale de philosophie,* No. 11 (1950).

MacCorquodale, K. and Meehl, P. E. On a distinction between hypothetical constructs and intervening variables. *Psychol. review,* vol. 55 (1948).

Margenau, H. Methodology of modern physics. *Philosophy of science,* vol. 2 (1935).

Northrop, F. S. C. *The logic of the sciences and the humanities.* New York, 1947.

O'Connor, D. J. Some consequences of Professor A. J. Ayer's verification principle. *Analysis,* vol. 10 (1950).

Popper, K. R. *Logik der Forschung.* Wien, 1935.

Reichenbach, H. *Philosophie der Raum-Zeit-Lehre.* Berlin, 1928.

Reichenbach, H. *Experience and prediction.* Chicago, 1938.

Reichenbach, H. *The theory of probability.* Univ. of California Press, 1949.

Spence, Kenneth W. The nature of theory construction in contemporary psychology. *Psychol. review,* vol. 51 (1944).

COMMENTS ON PROFESSOR HEMPEL'S "THE CONCEPT OF COGNITIVE SIGNIFICANCE"[1, 2]

GUSTAV BERGMANN
State University of Iowa

1. INTRODUCTORY

It will be best if I preface these brief comments by two statements of limitation concerning two matters Hempel mentions but which need not be taken up in discussing what is customarily meant by a criterion of meaning. The first of these (a) is more fundamental and, therefore, systematically prior to the problems at hand; the second (b) is technical in a sense in which the core of these problems is not.

(a) Like Hempel, I shall make use of the distinction between *analytic* and *synthetic* sentences and, also, of that between *logical* (structural) and *descriptive* (extralogical) terms. But though I shall not explicate these two dichotomies here, I do not wish to give the impression that I share Hempel's diffidence as to the possibility of establishing ''sharp dividing lines between statements of purely logical and statements of empirical significance''; that I do not consider as confusing an extension of the notion of analyticity which makes Carnap's reduction sentences analytic; or that I believe a philosophically significant notion of analyticity could and, perhaps, should be ''relativized'' in the sense indicated by Hempel. The gist of the matter, as I see it, is this. First, the two distinctions are completely clear and precise in that they can be made within pure syntax. Second, they can be made in a nontrivial manner in the sense in which, say, the setting aside of the logical terms by enumeration would be trivial. To say this is one thing. The familiar difficulties, how to decide whether in certain formal systems (language schemas) a given sentence is in fact analytic, or how we can ever be sure that such a schema is adequate for saying everything we want to say are another thing.

(b) Like Hempel, I believe that the technic known as the *partial interpretation of axiomatic systems* is useful in clarifying the logic or, if you please, the empirical significance of some terms. But I believe this to be true only for a relatively small class of highly specialized terms of theoretical physics such as 'electron' and 'nucleus' [1]. I suggest, therefore, that we fractionate our problem by first looking for a criterion of meaning that works satisfactorily for a language from which these very special terms have been temporarily expurgated. One reason why this seems wise is that the "coordination" of, say, the "empirical" term 'pressure', defined by means of scales, barometric columns, etc., which would still occur in our expurgated language, to the term 'pressure', as defined in, say, the classical atomic model, can be construed as a purely syntactical affair in exactly the same sense in which a Finnish-Swedish dictionary is for me, who understands neither of these two languages, a matter of syntax. The difference is that in the case at hand the translation rules are very complicated as well as rather limited, and that they are established by explicit agreement for the familiar purposes of scientific explanation. Also, this time we happen to understand one of the two languages; thus we understand, within the limits set by the dictionary, the other.

An objection sometimes raised against this way of construing the particle notions of theoretical physics is that it introduces an invidious distinction between, say, 'chair' and 'electron' and therefore, presumably, between chairs and electrons. Not surprisingly, the intellectual motive behind this objection is, more often than not, some sort of realism, anxious to insist that electrons are as "real" as chairs, if not more so. The proper way of dealing with this objection is, of course, to show, by the usual discursive and indirect method of analytical philosophy, that the proposed technic for construing certain terms does not in fact imply what these objectors fear it does. Clearly, the task is beyond the scope of these comments. It is worth noticing, though, that one step of the clarification consists in showing that even the particle notions of classical physics could, in principle, be introduced by explicit definitions. This, by the way, is also true of all the concepts of scientific psychology [10]. On the other hand, it seems that the philosophical puzzles that have been raised concerning the Schroedinger-Heisenberg theory cannot be dissolved without the technic of partial interpretation of axiomatic systems. With it, I believe I have shown that they disappear [4].

2. The Principle of Acquaintance

What Hempel now calls the *requirement of definability* is, if I understand him correctly, what analytical philosophers know as a *principle of acquaintance* [3]. I neglect, for this occasion, all problems connected with proper names; and I assume, without examining here the warrantedness of this assumption, what is part of the thesis of Russell's logical atomism, namely, that all descriptive terms of the ideal language are either proper names or predicates, including relational predicates. By 'ideal language' I mean that formally constructed schema in which, when it is once "interpreted", we could, with the exception mentioned and provided for in 1 (b), say "everything" [7, 9]. This being understood, the principle states that all descriptive predicates belong to, or can be explicitly (contextually) defined by means of, a class of terms that "name" characters which are "immediately observable." Or, as I prefer to put it, the principle maintains that a descriptive predicate of the *ideal language* is *either* syntactically primitive (undefined) and in this case belongs to a class C whose members name, upon interpretation, immediately observable characters *or* explicitly definable by means of members of C.

It is well to distinguish questions about the *status* of this principle from those about its *desirability* and, again, from those concerning its *adequacy*, i.e., whether an ideal language can actually be constructed in accordance with it. Presently I shall make a few remarks concerning its status. As to its desirability, I shall only say that the principle is closely related to an idea of philosophical analysis which I, and not I alone, have found very useful, for which I know no substitute, and which I am, therefore, loath to abandon. Very roughly speaking, the idea is that the clarification, or an essential part of the clarification, of the philosophical puzzles raised in connection with a term consists in providing for each sentence which contains it another sentence which does not contain it, so that the statement of equivalence between these two sentences is analytic. The insistence on the exclusive use of explicit definitions serves to secure this *eliminability,* which is no longer guaranteed when one admits other methods, such as Carnap's reduction chains, for "introducing" rather than, literally, defining descriptive predicates. Two supplementary remarks are in order.

(a) Even if, accepting the principle of acquaintance, we restrict ourselves to explicitly defined predicates, we cannot be sure of their eliminability in contexts which are not extensional [6]. But, as I have argued elsewhere [2], there is reason to believe that the ideal language is on the whole extensional. The qualification "on the whole" is necessary because of the nonextensional properties of 'des', a nondescriptive symbol to be interpreted as 'designates' and which the ideal language must contain since it must provide for the clarification of all philosophical puzzles and because this (or a similar) term is needed to provide for those concerning 'truth' and 'designation' [9].

(b) Even in an extensional schema that admits only explicit definitions, one would, unless a further condition is fulfilled, have to admit into the class C predicates that belong to different types; for instance, in addition to such color words as 'green', 'blue' and 'yellow' also the word 'color'. The condition is that no two undefined descriptive predicates are coextensive [5]. But then, I know of no two characters that are coextensive and at the same time simple enough for their names to be likely candidates for the status of undefined descriptive predicates in the ideal language.

3. The Adequacy of the Principle of Acquaintance

Hempel holds that the principle is inadequate or, as he puts it, "too restrictive." The reason he gives is the alleged necessity of introducing the so-called disposition terms by means of Carnap's reduction chains rather than by explicit definitions. To join the issue once more by means of the old illustration, let 'f_1' and 'f_2' stand for 'put into water' and 'dissolves' respectively. The predicate 'f_3' as explicitly defined by '$f_3(x) \equiv f_1(x) \supset f_2(x)$' is then not regarded as an acceptable analysis or, if you please, reconstruction of 'soluble (in water)'. If this rejection is at all justified, then it is of considerable importance; for (1) it affects, at least, all definitions whose main connective in the definiens is '$\supset$', and (2) a very large number of concepts — all those which, as one sometimes says, are operationally defined — would, if they are to be introduced by explicit definitions from a reasonably limited basis C, be of this form.

I am prepared to maintain that the principle of acquaintance is adequate and, not unnaturally in the light of what has just been said, I shall argue my case by arguing that 'f_3', *which I refrain from transliterating by the English word 'soluble'*, is all we need for an adequate analysis of this English word. Yet I know as well as the critics of explicit definition that 'f_3' is not a synonym of 'soluble', if for no other reason than that it could be truly predicated of a certain match which has been burned before it was ever put into water and of which nevertheless we wish to say that it is (was) not soluble. It must be clear by now that, in my opinion, the key to the problem cannot be found by more or less formal ingenuity. What is needed is reflection on what we mean by an adequate analysis or, to say *the same thing differently, reflection on the proper relation between* our ordinary language on the one hand and the ideal language or schema by means of which we analyze it on the other. But perhaps it is well to add here that, as I use the term and as I think it should be used by philosophers, what scientists speak and write is ordinary language.

Consider the English sentence 'If it rains and thunders then it necessarily rains'. What we do in one stage of the analysis of 'necessary', in the connotation it has in this sentence, is to point out that in the ideal language sentences of the form '$p.q \supset p.$' are analytic. What we don't do is look for a "synonym" of 'necessary' in the ideal schema. Again, in explicating the 'because' in the English sentence 'I have a headache because I smoked too much' we do not look for a synonym of 'because' but point out that in the ideal language three sentences of the same form as 'Whenever I smoke too much I have a headache', 'I smoked too much', 'I have a headache' respectively exemplify a syllogistic pattern. In this case, we notice, the analysis requires a whole group of sentences in the ideal schema. Generally, we must be prepared that what we call "translation" into the ideal schema is thus indirect, complex, and dependent on the whole context of this schema.

With this in mind, I propose to analyze the *particular* sentence 'The aforementioned match is (was) not soluble' by means of two sentences of the ideal schema, the first corresponding to 'This match is (was) wooden', the second to the *law* 'No wooden object is soluble'. At this point I shall be told that I have merely pushed the problem back one step since a law (whether or not one of the characters it conjoins is, as in this case, dispositional) cannot be adequately "translated" by a formal implication. In other words, I have run into the problem of counterfactuals which, as is well known and as Hempel again points out, is essentially the same as that of the disposition terms. Here is what I would reply to this criticism. What we mean when we state in ordinary English a law of nature conjoining two characters is indeed not adequately analyzed by '$f_1(x) \supset_x f_2(x)$' *alone,* even if, for the sake of the argument, we assume that 'f_1' and 'f_2' are undefined descriptive predicates and adequate "translations" of the English names of these characters. Such a law is adequately analyzed only by (1) giving a class of sentences of the ideal language that contains, in addition to '$f_1(x) \supset_x f_2(x)$', pairs of statements, such as '$f_1(a)$', '$f_2(a)$', which correspond to *nontrivial positive* instances of the formal implication; and (2) by pointing out the deductive connections among the sentences of this class. But if this is so, then 'f_3' may well be the second predicate in the formal implication that belongs to the class analyzing the English sentence 'No wooden object is soluble'. For it seems that the familiar deficiencies of 'f_3' will be made up completely by such triples as '$w(b)$', '$f_1(b)$', '$\sim f_2(b)$', with 'w' standing for 'wooden'. However, there are in this case still the *trivial negative* instances to be disposed of, as Hempel has pointed out to me in a private communication. In other words, something must be done about such triples as '$w(c)$', '$\sim f_1(c)$', '$\sim f_2(c)$', all three of which may presumably be true and which yet jointly imply the negation of '$w(x) \supset_x \sim f_3(x)$'. We must therefore add that (3) such triples are excluded from the class of sentences which we consider as an analysis of the law. Similar measures to avoid the snares of material implication will have to be taken for laws conjoining disposition terms. If, in a still more complicated case, 'f_1' and 'f_2' are defined by means of operators, then the analysis is not yet complete; "instances" for the "instances" will have to be added and the procedure repeated as often as is necessary.[3]

I conclude that reduction chains serve no useful purpose, that the principle of acquaintance is adequate, and that the problem of counterfactuals which now causes so much discussion is a pseudoproblem. Nor do I mind saying how surpassingly strange I find the readiness with which Hume's fundamental insight is jettisoned in this discussion by writers who are more or less in the empirical tradition. Historically, this is perhaps a sign that, for better or for worse, a certain type of positivism has run its course.

4. THE STATUS OF THE PRINCIPLE OF ACQUAINTANCE

When a philosopher proposes a criterion of meaning, some other philosopher is likely to ask him: "Can your criterion be applied to itself? Is, in particular, 'immediately observable' a member of your class C or, if not, can it be defined by means of it? If the answer is no, don't you convict yourself of talking nonsense? If the answer is yes, what does it prove? How can you be sure that your criterion is not too narrow? Is it not, therefore, at best a presupposition which you and your friends introduce dogmatically and sometimes, I fear, even unwittingly while, being such a presupposition, it can and must be argued?" Whoever poses such queries challenges the very idea of a meaning criterion rather than the adequacy of any particular one; to answer them is to clarify what I have called the status of our principle. Again, the right answer, or what I believe to be the right answer, is discursive and indirect. We arrive at it by reflecting on what we do, and why we do it, in philosophical analysis.

Its purpose is the clarification of the philosophical puzzles, those that are historically given to us and those we can think of ourselves. To achieve it, we begin by constructing *formally* that linguistic schema *L* which I call the ideal language. Then, by *informally discoursing about L in ordinary English* (which, by the way, we also used, for what else could we do, when we formally constructed *L*), we convince ourselves, as well as we may, of two things. First, if we "interpret" *L*, then we can, in principle, use it instead of ordinary English to say "everything" we wish to say. Second, we can now unravel the philosophical puzzles. Informal discourse of this kind is, therefore, an essential part of philosophy; it is, at least for me, the part that justifies the whole enterprise.

The principle of acquaintance or, for that matter, any such criterion, belongs to this informal discourse about *L*. But this does not make it, in any sense I could understand, a premise or presupposition. What happens, rather, is this. I *discover* that I can construct an *L* which is otherwise satisfactory and which I can understand after I have "interpreted" only those of its descriptive predicates which "name" what is "immediately observable". And, of course, I also discover that this circumstance is of strategic importance in clarifying many traditional puzzles; which, by the way, is the only good reason for calling this sort of thing a criterion or principle. But all this is only part of what needs to be said in reply to the tangle from which I started.

I have throughout put certain expressions, such as 'immediately observable', 'naming', and 'interpreting', between double quotes. This was done to indicate that I used them in informal philosophical discourse. Now one may well ask whether *L*, which is supposedly comprehensive in the sense that in it we could talk about everything, must not itself contain "translations" of these terms and whether, if it does, this whole idea of philosophical analysis is not vitiated by circularity. Both questions deserve an answer; also, they are relevant for the problem at hand since every meaning criterion that has ever been proposed contains crucially some of these terms, e.g., 'observable'. Let me then begin with 'observable' or, rather, with 'observing', as the former adds to the latter nothing but that connotation of 'possibility' which can be independently explicated by saying that what is possible is what can be expressed by a synthetic sentence of *L*. As to 'observing', I would say that *L* must indeed contain (descriptive) predicates which allow for a "translation" of the ordinary English verbs for our perceptual and other mental acts [8]. And I also grant that these verbs are by no means unproblematic; for to analyze them by "correctly" translating them into *L* is, in my opinion, the main key to some famous philosophical puzzles that revolve around them [2, 9]. However, and this is the point, in our informal discourse we use these verbs not as they are used in the traditional puzzles but in their ordinary sense, which does not lead to any perplexity; and we find that by thus using them in talking about *L* we can dissolve the perplexities of these puzzles. There is, as far as I can see, nothing vitiatingly circular in this procedure. What goes for 'observing', goes also for such "semantical" words as 'naming' and 'interpreting'. But some may grant the force of these remarks and yet demur against my saying, even though in double quotes, that *L* can speak about "everything". For does it, or could it conceivably say what I just said about it? Thus pressed, I would not mind amending my statement to the effect that by talking about *L* we can clarify everything there is to be clarified, including this talking itself. The very idea of talking about something, for instance, is clarified by the "semantical" part of *L*. This much I would say to such a questioner. Silently I would wonder whether he has really grasped the distinction, which is to me a quite unproblematic matter of common sense, between using a symbol on the one hand and mentioning it on the other.

Historically, this account of the respective roles of formal and informal discourse in philosophical analysis is, it seems to me, only another explication of an old idea. G. E. Moore, I believe, expressed it by his doctrine of common sense; Wittgenstein, in an unfortunately paradoxical and prohibitive manner, by his famous distinction between what can and what cannot be said. And I should also like to think that with his recent distinction between questions internal and questions external to a linguistic framework Carnap[5] approaches again, no matter how hesitatingly, this common ground. But I am not at all sure of this since he still disparages all external questions and thus also the two all-important ones of the comprehensiveness of the formalism and of its usefulness in clarifying the traditional puzzles, so that we may understand them and do not need to dismiss them by decree.

NOTES

[1] I am indebted to Professor Hempel for letting me use the manuscript of his paper in editing these comments.

[2] Since I shall refer to several of my papers I list them here and cite them in the text by bracketing the numbers under which they appear in this list. 1. ''Outline of an empiricist philosophy of physics'', *Amer. J. of Physics,* 11, 1943; 2. ''A positivistic metaphysics of consciousness'', *Mind,* 54, 1945; 3. ''Remarks on realism'', *Phil. Sci.,* 13, 1946; 4. ''The logic of quanta'', *Amer. J. of Physics,* 15, 1947; 5. ''Conditions for an extensional elementaristic language'', *Analysis,* 8, 1948; 6. ''Contextual definitions in nonextensional contexts'', *J. Symb. Logic,* 13, 1948; 7. ''Two criteria for an ideal language'', *Phil. Sci.,* 16, 1949; 8. ''Professor Ayer's analysis of knowing'', *Analysis,* 9, 1949; 9. articles ''Logical Positivism'' and ''Semantics'' in *History of Philosophical Systems* (ed. V. Ferm), Philosophical Library, New York, 1950; 10. ''The logic of psychological concepts'', *Phil. Sci.,* 18, 1951.

[3] In an interesting paragraph of a very recent paper Hempel himself argues substantially the reasonableness of such an analysis through *context:* ''Problems and changes in the empiricist criterion of meaning'', *Rev. internat. de philosophie,* no. 11, 1950 (p. 59). However, 'context', as philosophers use it, is a very ambiguous term and provides the occasion for another remark that should, perhaps, be made in discussing a meaning criterion.

Some think that even if, say, 'mass' could be analyzed by explicit definitions in some such way as I propose, this would still not exhaust the meaning of 'mass'. The gist of this criticism is that the *laws in which a term occurs* are part of its meaning. E.g., the constancy of mass is, upon this view, a part of the meaning of 'mass' in Newtonian physics. This criticism can be disposed of by distinguishing between two of the many connotations of the English word 'meaning'. The one refers to the way a term is introduced; the other to the laws in which it occurs. I have repeatedly and again in a recent paper [10] insisted on this distinction. Idealistic and pragmatist positions attempt to absorb the first (introduction) of these two meanings of 'meaning' into the second (context); in discussing a ''meaning criterion'' we are concerned only with the first. See also M. Brodbeck, ''The new rationalism: Dewey's theory of induction'', *J. of Phil.,* 46, 1949.

[4] 'Descriptive predicate' is here a purely syntactical term. See the remarks in section 1 (a) above.

[5] ''Empiricism, semantics, and ontology'', *Rev. internat. de philosophie,* no. 11, 1950.

NATURAL AND SCIENTIFIC LANGUAGE

JOHN LOTZ
Columbia University

Natural languages and calculi (constructed scientific languages) differ more basically than is generally assumed by logisticians, and the supposed isomorphy, e.g., between English and the language of *Principia Mathematica,* does not hold.

A natural language — the cultural possession of every known human community — has no limitations for any semantic or pragmatic function; it can express narration, conviction, anger, irony, command, evaluation, truth, falsehood, etc.; it is a *universal* language in the sense that there is nothing which in principle cannot be expressed in it. But just because of these broad social and semiotic implications, the semantic references of a natural language are vague. This is the reason why, when a language is developed for scientific purposes, the special conventions of a calculus are introduced. The transition from a natural language to a scientific calculus, involving the restriction of the full-scale functions of the former to a single one of the latter, is a central problem of philosophy. On the other hand, the mere substitution of mathematical symbols for natural language symbols for reasons of clarity, economy of thought, etc., such as when one transposes "Every human is mortal" into $(x)\ (\text{Human}\ (x) \rightarrow \text{Mortal}\ (x))$, or $\Pi\ Hm$—although it might be important for technical reasons or as a matter of convenience—seems to be of a subordinate importance.

Thus, a scientific calculus is always dependent on a natural language; the calculus is always *parasitic.* This fact is overlooked in the treatment of the "meta"-problems, which concentrates solely upon the analysis of the formalized symbolism and disregards the manner in which this symbolism has been derived from a natural language. Also, speakers, writers, and discussants of problems of mathematical calculi always use a natural language with occasional specific symbolism, but not more than would be used by any other specialized group, as e.g., farmers, brokers, or football players. Furthermore, the specific characteristics of scientific calculi cannot be automatically transferred to the domain of natural languages; there is, for instance, no use in speaking of axioms, inference, etc., in a natural language like English, where there are no rules of logical deduction or logical necessity in the purely lingual usage, and any sequence which pleases the speaker can be used if it conforms with the syntactic rules of English. Nor can the assurance of mathematical logicians that commands, etc., can be expressed within the present framework of a calculus be taken at face value. A transposition of the sentence "Shut up!" can no more be equaled by the proposition "The speaker wishes the addressee to cease speaking" (except for a similar usage of symbolic material) than the sentence "X has a toothache" is equaled by the actual toothache of X. Should a language be constructed, however, which would have the same field of semantic and pragmatic reference as a natural language (as has been done in artificial languages or in pasigraphic systems *à la* Chinese), it would be simply a transposition of a natural language and would imply the same scientific problem as the relation of Esperanto to English and would not pose any new problem for the philosophy of language. Therefore, the contention that a child could learn the language of *Principia Mathematica* instead of English cannot be accepted. How would P. M. nursery rhymes or the emotional outbursts of a child sound?

It would seem necessary to reinvestigate the relation between natural language and the calculi.

PART 3

THE ROLE OF "ABSTRACT OBJECTS" IN SCIENCE

SEMANTICS AND ABSTRACT OBJECTS

W. V. Quine
Harvard University

Let me begin by deploring two tendencies in the philosophy of language. One is a tendency to confuse meaning with reference; the other is a tendency to widen excessively the category of singular terms.

The tendency to confuse meaning with reference is the one which Frege took pains to combat, though it was, even in Frege's time, less widespread than the other. There is a long tradition for the distinction between connotation and denotation, or between intension and extension; and this is a distinction squarely between meaning and reference. At the level of singular terms, however, meaning and reference were less clearly distinguished. Perhaps there has been a connection between Mill's doctrine that proper names have no connotation, but only reference, and the popular but mistaken inclination to identify the meaning of a proper name with the object named.

Another type of expression whose meaning has commonly been confused with its reference is the statement. Statements have frequently been treated as names of propositions, these latter being construed as entities of a sort better describable as *meanings* of statements. The confusion in the case of proper names and that in the case of statements are opposite; the name is thought of as meaning the named object, for lack of connotation, whereas the statement is thought of as naming its meaning, for want of anything better to name.

Frege kept meaning and reference sharply separated in all cases. He construed a general term as *naming* its extension — the class of all things of which the term is true — and he construed a statement as naming its truth value. On the other hand, he took the meaning (*Sinn*) of a statement to be the proposition, this being analogous to the intension or idea which is the meaning of a general term. For singular terms, likewise, he recognized meanings, distinct from the objects named.

Actually general terms and statements are themselves singular terms for Frege, since he treats each of these as a name of a single entity — a class or a truth value. So Frege is a prime example of the second tendency I promised to deplore — that of excessively widening the category of singular terms. But let me postpone this matter until I have said what I have to say about meaning.

When the cleavage between meaning and reference is properly heeded, the problems of what is loosely called semantics become separated into two provinces so fundamentally distinct as not to deserve a joint appellation at all. I shall call these the *theory of meaning* and the *theory of reference.* 'Semantics' would be a good name for the theory of meaning, were it not for the fact that some of the best work in so-called semantics, notably Tarski's, belongs to the theory of reference. The main concepts in the theory of meaning, apart from meaning itself, are *synonymy* (or sameness of meaning), *significance* (or possession of meaning), and *analyticity* (or truth by virtue of meaning). Another is *entailment,* or analyticity of the conditional. The main concepts in the theory of reference are *naming, truth* and *denotation* (or 'truth-of', in the sense in which the term 'green' denotes or is true of each green thing). Another is *extension,* or the class of denoted objects; also the notion of *values* of variables. The theory of reference is the locale of the so-called semantical paradoxes, such as Grelling's and Berry's and the Epimenides.

The theory of meaning is not troubled with paradox, but it has troubles of a different order. The most conspicuous question is as to the nature of its objects: what sort of things are meanings? They are evidently intended to be ideas, somehow — mental ideas for some semanticists, Platonic ideas for others. Objects of either sort are so elusive, not to say debatable, that there seems little hope of erecting a significant science about them. It is not even clear, granted meanings, when we have two and when we have one; it is not clear when expressions should be regarded as alike in meaning and when they should not. If a standard of synonymy should be arrived at, we may reasonably expect that the appeal to meanings as entities will not have played a very useful part in the enterprise.

A felt need for meant entities may derive from an earlier failure to appreciate that meaning and naming are distinct. Once the theory of meaning is sharply separated from the theory of reference, it is a short step to recognizing as the business of the theory of meaning simply the synonymy of expressions, the meaningfulness of expressions, and the analyticity or entailment of statements; meanings themselves, as obscure intermediary entities, may well be abandoned.[1] This is a step that Frege did not take.

The notions of synonymy, meaningfulness, analyticity, and entailment are connected in intimate ways. Statements are synonymous, in a broad sense, if their biconditional is analytic, or in other words if they entail each other. Singular terms are synonymous if their identity is analytic. Predicates are synonymous if, when they are applied to variables, their universally quantified biconditional is analytic. An expression is meaningful if synonymous with itself. A statement is analytic if synonymous, in our broad sense, with some arbitrarily chosen specimen — say '$(x)(x = x)$'. But there is great difficulty in tying this well-knit group of concepts down to terms that we really understand. The theory of meaning, even with the elimination of the mysterious meant entities, strikes me as in a comparable state to theology — but with the difference that its notions are blithely used in the supposedly most scientific and hard-headed brands of philosophy.

As an empiricist I consider that the cognitive synonymy of statements should consist in sameness of the empirical conditions of their confirmation. A statement is analytic when its operational condition of verification is, so to speak, the null condition. But I am using terms here which we cannot pretend to understand until we have made substantial progress in the theory of confirmation. Considering what good hands the problem of confirmation is in, it may seem that optimism is in order. However, if synonymy and analyticity and the rest are to be made sense of only in terms of an eventual account of confirmation, I think philosophers are tending to be insufficiently chary of the circularity involved in resting their eventual account of confirmation upon such concepts as synonymy and analyticity.

So much for meaning. Now let me return to the theory of reference, to deplore the practice by Frege and others of excessively widening the category of terms that name. Once the theory of meaning is well separated from that of naming, there is no need to seek a named object in order to insure meaningfulness. I prefer to consider that naming is the function of singular terms in the original narrow sense, excluding general terms and statements. General terms and statements have referential functions of their own, but of a different type than meaning; a general term is *true of* this and that object and *false of* this and that object, and a statement is *true* or *false*. A general term *has* its extension, viz. the class of all things of which it is true, and a statement *has* its truth value; but there is no need to treat the general term as a name of its extension, nor the statement as a name of its truth value. Names can be formed for classes and for truth values, as for anything else; but I prefer not to regard the general terms and statements as names at all. On this point, though by no means in general, I agree with Carnap's *Meaning and Necessity.*

Why I want to keep general terms and statements out of the category of singular terms will be clearer later. Meanwhile let us consider wherein, in principle, naming consists. I shall assume that we use a language in which quantification is fundamental. Clearly, then, the things which we must recognize as *there being,* in order for our discourse to be true, are just the things which must fall within the range or ranges of our variables of quantification in order that the statements we affirm come out true. This is obvious from the meaning of the quantifiers: 'each object x is such that', 'there is something x such that'. It follows that a term need be regarded as a name of something only if the term is always substitutable for the variable of a universal quantification,[2] without leading from truth to falsehood or nonsense.

To say that Frege treats general terms as names of classes and statements as names of truth values, then, is simply to say that he provides for substitution of general terms for quantified class variables and substitution of statements for quantified truth-value variables.

The disadvantage of this procedure is that it leads to a false accounting of the ontological presuppositions of most of our discourse. When we say that some dogs are black, $(\exists x)$ $(x$ is a dog $\cdot$ x is black), we explicitly admit some black dogs into our universe but do not commit ourselves to such abstract entities as dogkind or the class of black things; hence it is misleading to construe the words 'dog' and 'black' as names of such entities.

Classes are genuinely presupposed in discourse which quantifies over class variables. There are occasions which call quite directly for this sort of discourse; e.g., where we want to say that some zoölogical species are cross-fertile. Another is where we want to define ancestor in terms of parent, by Frege's method: x is ancestor of y if x belongs to *every class* which contains y and all parents of its own members. There is thus serious motive for quantification over classes; and, insofar, there is a place for singular terms which name classes — such singular terms as "dogkind" and "the class of Napoleon's ancestors". But let us not read such terms into positions where no appeal to classes is involved.

I would distinguish, therefore, between:

$$(\exists x)\ (x \text{ is-a-dog} \;.\; x \text{ is-black}) \quad \text{and} \qquad (1)$$

$$(\exists x)\ (x \varepsilon \text{ dogkind} \;.\; x \varepsilon \text{ class-of-black-things}). \qquad (2)$$

(2) is a case of the form:

$$(\exists x)\ (x \epsilon y \;.\; x \epsilon z), \quad \text{or:} \quad (\exists x)\ (x \epsilon \alpha \;.\; x \epsilon \beta).$$

The "dogkind" and "class-of-black-things" in (2) are singular terms, capable of occurring in positions appropriate to variables of quantification. On the other hand the general terms or predicates "is-a-dog" and "is-black", in (1), are not to be regarded as meaningfully substitutable for variables. I would represent the form of (1) merely by a *schema* '$(\exists x)\ (Fx \;.\; Gx)$', in which the '$F$' and '$G$' are purely diagrammatic and do not admit of quantification.

I grant that (1) and (2) as wholes are equivalent statements. But they differ in that (1) belongs to the part of language which is neutral on the question of class existence, whereas (2) is a degenerate specimen of the higher part of language. It is degenerate merely in that it happens as a whole to be equivalent to (1).

The bulk of logical reasoning takes place on a level which does not presuppose abstract entities. Such reasoning proceeds mostly by quantification theory, the laws of which can be represented through schemata involving no quantification over class variables. Much of what is commonly formulated in terms of the algebra of classes or the algebra of relations actually needs no such things as classes and relations, and can be easily reformulated schematically within quantification theory plus perhaps identity theory.

Thus I consider it a defect in an all-purpose formulation of the theory of reference if, like Frege's, the formulation represents us as referring to abstract entities from the very beginning rather than only where there is a real purpose in such reference. Hence my wish to keep general terms distinct from abstract singular terms.

I am not here arguing over-all repudiation of abstract entities. In discussing the theory of meaning I did urge the uselessness of meanings as entities. My thesis there was, in effect, that no gain is to be sought in quantifying over such alleged entities as meanings. But quantification over classes or other abstract entities is the essence of set theory and indeed of most of classical mathematics; and mathematics is, figuratively as well as literally, something to reckon with. I urge only that we put no obstacles in the way of distinguishing clearly between that part of discourse which depends squarely on an ontology of abstract entities and that part which does not.

In depriving general terms or predicates of the virtues of names I do not thereby deny that there are certain classes connected with them otherwise than in the fashion of being named. On the contrary, in that part of the theory of reference which has to do with classes there is occasion to speak of the *extension of* a general term or predicate — the class of all things of which the predicate is true. There is occasion in that part of the theory of reference to assume such classes and to quantify over them. One such occasion arises when in the theory of reference we treat the topic of validity of schemata of pure quantification theory; for a quantificational schema is valid when it comes out true for all assignments of classes as extensions of the schematic predicate letters. The general theory of quantificational validity thus appeals to classes, but the individual statements represented by the schemata of quantification theory need not; the statement '$(\exists x)$ (x is a dog . x is black)' involves, of itself, no appeal to the abstract extension of a predicate.

Similarly there is occasion in the theory of validity to speak of truth values of statements, e.g., in defining truth-functional validity. But there is no need to treat statements as names of these values, nor as names at all. When we simply affirm a statement we do not thereby appeal to any such entity as truth values, unless the statement happens to have that special subject matter.

It can indeed prove convenient and elegant in special systems to reconstrue statements as names — perhaps of 0 and 1, as in Church's system. This is perhaps better regarded as a matter of making names of 0 and 1 serve the purpose of statements, for the special system; and I have no quarrel with it. Similarly Frege may be represented as making his singular terms, plus membership, do the work of general terms; and with this again, as a means merely of absorbing lower logic into a particular system of higher logic for the sake of elegance, there is no quarrel. What I object to is projection of these special expedients into the general theory of reference. It is preferable in the general theory of reference to proceed in such a way as not to impute special ontological presuppositions to portions of discourse which are innocent of them.

Even in the higher theory of validity it happens that the appeal to truth values of statements and extensions of predicates can finally be eliminated. For, truth-functional validity can be redefined by the tabular test of computation, and validity in quantification theory can be redefined simply by appeal to the rules of proof (since Gödel has proved them complete). Here is a good example of the elimination of ontological presuppositions, in one particular domain.

In general it is important, I think, to show how the purposes of a certain segment of mathematics can be met with a reduced ontology, just as it is important to show how an erstwhile nonconstructive proof in mathematics can be accomplished by constructive means. The interest in progress of this type is no more dependent upon an out-and-out intolerance of abstract entities than it is upon an out-and-out intolerance of nonconstructive proof. The important thing is to understand our instrument; to keep tab on the diverse presuppositions of diverse portions of our theory, and reduce them where we can. It is thus that we shall best be prepared to discover, eventually, the over-all dispensability of some assumption that has always rankled as *ad hoc* and unintuitive.

[1] When I presented this paper at the meeting of the Institute for the Unity of Science in April, 1950, I had no thought of publication. Eight months later, at the Toronto meeting of the American Philosophical Association, I presented my paper ''Two dogmas of empiricism,'' and it appeared in the *Philosophical Review* in January, 1951. I now observe in reading proof that the above nineteen lines recurred almost verbatim in ''Two dogmas,'' pp. 22 f, though in the main the two papers are unlike in content.

[2] With appropriate type of variable, in case there are distinctions.

COMMENTS ON PRECEDING PAPER OF W. V. QUINE

Max Black
Cornell University

"When the cleavage between meaning and reference is properly heeded," says Professor Quine, "the problems of what is loosely called semantics become separated into two provinces so fundamentally distinct as not to deserve a joint appellation at all." If this is right, it would be helpful to be told on what principle the separation is to be made. What, if anything, do questions about reference have in common?

Frege's doctrines will not help us in answering this question. Indeed it seems to me a mistake to try to connect Quine's partition of semantics with the distinction between *Bedeutung* and *Sinn*. For one thing, each of these everyday German words would be translated by a dictionary as equivalent to "meaning," and Frege himself thought of them as referring to different aspects of meaning; his work would have been hamstrung by any prohibition against discussing the relations between them in one and the same context. Would his paper on "*Sinn und Bedeutung*" need to be relegated to yet a third "province" if Quine's admonition was heeded?

It would have annoyed Frege to be told that "he construed a general term as *naming* its extension—the class of all things of which the term is true." Time and again Frege made it plain that a grammatical predicate stood for a *Begriff* (concept), that is to say for one of a special kind of first-level *function* having only truth-values as its values. In order to name the extension of a concept he used not a general term but a special complex sign (roughly equivalent to a class-name in *Principia Mathematica*). He made the sharpest of distinctions between an object (*Gegenstand*) and a function; it is therefore misleading, to say the least, to claim that Frege took a general term to be "a name of a single entity." And for Frege the relation between a concept and the things "falling under" that concept, as he always said, was emphatically not the relation of *Bedeutung*.

The question before us, however, is not what Frege meant by "*Bedeutung*" but rather what Quine means by "reference." "The main concepts in the theory of reference," he says, "are *naming*, *truth*, and *denotation* (or 'truth-of,' in the sense in which the term 'green' denotes or is true of each green thing). Another is *extension*, or the class of denoted objects; also the notion of *values* of variables.'' Is the idea behind this list that all of these notions are used to say what is the case, rather than what words and signs *mean*? No doubt the semanticist (unfortunate word) need not bother whether 'Ossian' was any real person's name any more than he need worry about the truth of the *Iliad* or wonder what song the Syrens sang. But he is not expected to answer specific questions about meaning either. If his job is to *analyze* the meanings of ''synonymy,'' ''significance,'' and so forth, it is not clear that he can dispense with talk about truth-conditions, i.e., about what *would* be the case. When Quine says that ''the cognitive synonymy of statements should consist in sameness of the empirical conditions of their confirmation'' he comes close to saying as much himself.

However, there is little profit in haggling about a proposal to divide a field of study into separate provinces. The proof of the semantics is the axiomatization thereof, and when Quine has provided an analytical account of synonymy and related notions making no use of notions belonging to the theory of reference I shall be the first to cheer. It is good that the attempt should be made. On the other hand, I cannot see anything that needs to be ''deplored'' in ''the practice of Frege and others'' of using names and descriptions to stand for meanings. It would be naive to suppose that an expression like ''the meaning of *S*'' was in no need of logical anchorage to clearer idioms. But as much could be said of any technical expression in semantics; I cannot see that ''naming'' is especially vulnerable to criticism.

As to the alleged ''ontological presuppositions'' of such discourse, I wish it were possible to use a more aseptic description of what Quine has in mind. The formula, ''To be is to be the value of a variable''[1] is too attractively paradoxical not to engender confusion. Like other paradoxes, its charm depends upon its patent falsity. Anybody who takes it at face value by substituting for the second occurrence of the words ''to be'' in the formula itself will derive the Stein-ish formula ''To be is to be the value of a variable the value of a variable.'' And if the formula is applied to such a sentence as ''There are wild Pandas,'' the result is the sentence ''Wild Pandas are values of variables.'' This is hardly what can be meant by saying there are wild Pandas. A variable is a certain sign, like 'x', having a distinctive and familiar set of uses in logical and mathematical notation; it would seem, then, that the existence of Pandas depends upon the existence of signs like '*x*' and '*y*'. Could it be that there were no wild Pandas before logicians began using letters from the end of the alphabet as place-holders?

If we take the reference to variables less strictly, we get something like the following interpretation: To find out whether somebody "countenances" wild Pandas (to use another charming idiom), find out whether he is prepared to use a pronoun in place of "a wild Panda" or, better still, whether he is prepared to substitute "something." A bedtime story might well begin: "A phoenix approached the bed where John lay sleeping. *He* growled softly, so John knew *something* was in the room." Must the storyteller believe in phoenixes? Is the mathematician who talks about parallel lines intersecting at a point at infinity "ontologically committed" to there being a point at infinity? Surely it is impossible to settle such questions by any formal criterion that relies upon the use of expressions like 'there is' or 'he' or 'something.' There is no formal test to determine the difference between make-believe and serious assertion, or between a mere manner of speech and an "ontological commitment"—whatever that may be.

I cannot see that anything that has traditionally been called "ontology" is involved in the technical question of how economical one can be in using quantification in a rigorous formal development of semantics. The technical question has considerable interest in its own right and has everything to gain from being set free from its supposed philosophical implications.

---

[1] See Quine's article, "Designation and Existence," *Journal of Philosophy*, 1939. Cf. "To be is, purely and simply, to be the value of a variable" (*Review of Metaphysics*, 1948, p. 32).

THE NEED FOR ABSTRACT ENTITIES IN SEMANTIC ANALYSIS

ALONZO CHURCH
Princeton University

We distinguish between a *logistic system* and a *formalized language* on the basis that the former is an abstractly formulated calculus for which no interpretation is fixed, and thus has a syntax but no semantics; but the latter is a logistic system together with an assignment of meanings to its expressions.

As primitive basis of a logistic system it suffices to give, in familiar fashion: (1) The list of primitive symbols or *vocabulary* of the system (together usually with a classification of the primitive symbols into categories, which will be used in stating the formation rules and rules of inference). (2) The *formation rules*, determining which finite sequences of primitive symbols are to be *well-formed* expressions, determining certain categories of well-formed expressions — among which we shall assume that at least the category of *sentence* is included—, and determining (in case *variables* are included among the primitive symbols) which occurrences of variables in a well-formed expression are *free* occurrences and which are *bound* occurrences.[1] (3) The transformation rules or *rules of inference*, by which from the *assertion* of certain sentences (the *premisses*, finite in number) a certain sentence (the *conclusion*) may be *inferred*. (4) Certain asserted sentences, the *axioms*.

In order to obtain a formalized language it is necessary to add, to these *syntactical rules* of the logistic system, *semantical rules* assigning meanings (in some sense) to the well-formed expressions of the system.[2] The character of the semantical rules will depend on the theory of meaning adopted, and this in turn must be justified by the purpose which it is to serve.

Let us take it as our purpose to provide an abstract theory of the actual use of language for human communication — not a factual or historical report of what has been observed to take place, but a norm to which we may regard every-day linguistic behavior as an imprecise approximation, in the same way that e.g. elementary (applied) geometry is a norm to which we may regard as imprecise approximations the practical activity of the land-surveyor in laying out a plot of ground, or of the construction foreman in seeing that building plans are followed. We must demand of such a theory that it have a place for all observably informative kinds of communication — including such notoriously troublesome cases as belief statements, modal statements, conditions contrary to fact — or at least that it provide a (theoretically) workable substitute for them. And solutions must be available for puzzles about meaning which may arise, such as the so-called "paradox of analysis."

There exist more than one theory of meaning showing some promise of fulfilling these requirements, at least so far as the formulation and development have presently been carried. But the theory of Frege seems to recommend itself above others for its relative simplicity, naturalness, and explanatory power —or, as I would advocate, Frege's theory as modified by elimination of his somewhat problematical notion of a function (and in particular of a *Begriff*) as *ungesättigt*, and by some other changes which bring it closer to present logistic practice without loss of such essentials as the distinction of sense and denotation.

This modified Fregean theory may be roughly characterized by the tendency to minimize the category of *syncategorematic* notations — i.e., notations to which no meaning at all is ascribed in isolation but which may combine with one or more meaningful expressions to form a meaningful expression[3] — and to reduce the categories of meaningful expressions to two, (proper) *names* and *forms*, for each of which two kinds of meaning are distinguished in a parallel way.

A name, or a *constant* (as we shall also say, imitating mathematical terminology), has first its *denotation*, or that of which it is a name.[4] And each name has also a *sense* — which is perhaps more properly to be called its meaning, since it is held that complete understanding of a language involves the ability to recognize the sense of any name in the language, but does not demand any knowledge beyond this of the denotations of names. (Declarative) *sentences*, in particular, are taken as a kind of names, the denotation being the *truth-value* of the sentence, *truth* or *falsehood*, and the sense being the *proposition* which the sentence expresses.

A name is said to *denote* its denotation and to *express* its sense, and the sense is said to be *a concept of* the denotation. The abstract entities which serve as senses of names let us call *concepts* — although this use of the word 'concept' has no analogue in the writings of Frege, and must be carefully distinguished from Frege's use of 'Begriff.' Thus anything which is or is capable of being the sense of some name in some language, actual or possible, is a concept.[5] The terms *individual concept, function concept,* and the like are then to mean a concept which is a concept of an individual, of a function, etc. A *class concept* may be identified with a *property,* and a *truth-value concept* (as already indicated) with a proposition.

Names are to be meaningful expressions without free variables, and expressions which are analogous to names except that they contain free variables, we call forms (a rather wide extension of the ordinary mathematical usage, here adopted for lack of a better term).[6] Each variable has a *range,* which is the class of admissible *values* of the variable.[7] And analogous to the denotation of a name, a form has a *value* for every system of admissible values of its free variables.[8]

The assignment of a value to a variable, though it is not a syntactical operation, corresponds in a certain way to the syntactical operation of substituting a constant for the variable. The denotation of the substituted constant represents the value of the variable.[9] And the sense of the substituted constant may be taken as representing a *sense-value* of the variable. Thus every variable has, besides its range, also a *sense-range,* which is the class of admissible sense-values of the variable. And analogous to the sense of a name, a form has a *sense-value* for every system of admissible sense-values of its free variables.[10]

The following principles are assumed:[11] (i) Every concept is a concept of at most one thing. (ii) Every constant has a unique concept as its sense. (iii) Every variable has a non-empty class of concepts as its sense-range. (iv) For any assignment of sense-values, one to each of the free variables of a given form, if each sense-value is admissible in the sense that it belongs to the sense-range of the corresponding variable, the form has a unique concept as its sense-value. (v) The denotation of a constant is that of which its sense is a concept. (vi) The range of a variable is the class of those things of which the members of the sense-range are concepts. (vii) If S, s_1, s_2, $\ldots$, s_m are concepts of A, a_1, a_2, $\ldots$, a_m respectively, and if S is the sense-value of a form F for the system of sense-values s_1, s_2, $\ldots$, s_m of its free variables x_1, x_2, $\ldots$, x_m, then the value of F for the system of values a_1, a_2, $\ldots$ a_m of x_1, x_2, $\ldots$, x_m is A. (viii) If C' is obtained from a constant C by replacing a particular occurrence of a constant c by a constant c' that has the same sense as c, then C′ is a constant having the same sense as C.[12] (ix) If C′ is obtained from a constant C by replacing a particular occurrence of a constant c by a constant c′ that has the same denotation as c, then C′ is a constant having the same denotation as C.[13] (x) If C′ is obtained from a constant C by replacing a particular occurrence of a form f by a form f′ that has the same free variables as f, and if, for every admissible system of sense-values of their free variables, f and f′ have the same sense-value, then C′ is a constant having the same sense as C.[12] (xi) If C′ is obtained from a constant C by replacing a particular occurrence of a form f by a form f′ that has the same free variables as f, and if, for every system of values of their free variables which are admissible in the sense that each value belongs to the range of the corresponding variable, f and f′ have the same value, then C′ is a constant having the same denotation as C.[13] (xii) If $x_1, x_2, \ldots, x_m$ are all the distinct variables occurring (necessarily as bound variables) in a constant C, if $y_1, y_2, \ldots, y_m$ are distinct variables having the same sense-ranges as $x_1, x_2, \ldots, x_m$ respectively, and if C′ is obtained from C by substituting $y_1, y_2, \ldots, y_m$ throughout for $x_1, x_2, \ldots, x_m$ respectively, then C′ is a constant having the same sense as C. (xiii) If $x_1, x_2, \ldots, x_m$ are the distinct variables occurring in a constant C, if $y_1, y_2, \ldots, y_m$ are distinct variables having the same ranges as $x_1, x_2, \ldots, x_m$ respectively, and if C′ is obtained from C by substituting $y_1, y_2, \ldots, y_m$ throughout for $x_1, x_2, \ldots, x_m$ respectively, then C′ is a constant having the same denotation as C. (xiv) The result of substituting constants for all the free variables of a form is a constant, if the sense of each substituted constant belongs to the sense-range of the corresponding variable.[12] (xv) The sense of a constant C thus obtained by substituting constants $c_1, c_2, \ldots, c_m$ for the free variables $x_1, x_2, \ldots, x_m$ of a form F is the same as the sense-value of F when the senses of $c_1, c_2, \ldots, c_m$ are assigned as the sense-values of $x_1, x_2, \ldots, x_m$.

To these must still be added principles which are similar to (viii)–(xv), except that substitution is made in forms instead of constants, or that forms and variables as well as constants are substituted for the free variables of a form. Instead of stating these here, it may be sufficient to remark that they follow if arbitrary extensions of the language are allowed by adjoining (as primitive symbols) constants which have as their senses any concepts that belong to sense-ranges of variables in the language, if the foregoing principles are assumed to hold also for such extensions of the language, and if there is assumed further: (xvi) Let an expression F contain the variables $x_1, x_2, \ldots, x_m$; and suppose that in every extension of the language of the kind just described and for every substitution of constants $c_1, c_2, \ldots, c_m$ for the variables $x_1, x_2, \ldots, x_m$ respectively, if the sense of each constant belongs to the sense-range of the corresponding variable, F becomes a constant; then F is a form having $x_1, x_2, \ldots, x_m$ as its free variables.

To those who find forbidding the array of abstract entities and principles concerning them which is here proposed, I would say that the problems which give rise to the proposal are difficult and a simpler theory is not known to be possible.[14]

To those who object to the introduction of abstract entities at all I would say that I believe that there are more important criteria by which a theory should be judged. The extreme demand for a simple prohibition of abstract entities under all circumstances perhaps arises from a desire to maintain the connection between theory and observation. But the preference of (say) *seeing* over *understanding* as a method of observation seems to me capricious. For just as an opaque body may be seen, so a concept may be understood or grasped. And the parallel between the two cases is indeed rather close. In both cases the observation is not direct but through intermediaries — light, lens of eye or optical instrument, and retina in the case of the visible body, linguistic expressions in the case of the concept. And in both cases there are or may be tenable theories according to which the entity in question, opaque body or concept, is not assumed, but only those things which would otherwise be called its effects.

The variety of entities (whether abstract or concrete) which a theory assumes is indeed one among other criteria by which it may be judged. If multiplication of entities is found beyond the needs of the workability, simplicity, and generality of the theory, then the razor shall be applied.[15] The theory of meaning here outlined I hold exempt from such treatment no more than any other, but I do advocate its study.

Let us return now to our initial question, as to the character of the semantical rules which are to be added to the syntactical rules of a logistic system in order to define a particular formalized language.

On the foregoing theory of meaning the semantical rules must include at least the following: (5) *Rules of sense,* by which a sense is determined for each well-formed expression without free variables (all such expressions thus becoming names). (6) *Rules of sense-range,* assigning to each variable a sense-range. (7) *Rules of sense-value,* by which a sense-value is determined for every well-formed expression containing free variables and every admissible system of sense-values of its free variables (all such expressions thus becoming forms).

In the case of both syntactical and semantical rules there is a distinction to be drawn between *primitive* and *derived* rules, the primitive rules being those which are stated in giving the primitive basis of the formalized language, and the derived rules being rules of similar kind which follow as consequences of the primitive rules. Thus besides primitive rules of inference there are also derived rules of inference, besides primitive rules of sense also derived rules of sense, and so on. (But instead of "derived axioms" it is usual to say *theorems.*)

A statement of the denotation of a name, the range of a variable, or the value of a form does not necessarily belong to the semantics of a language. For example, that 'the number of planets' denotes the number nine is a fact as much of astronomy as it is of the semantics of the English language, and can be described only as belonging to a discipline broad enough to include both semantics and astronomy. On the other hand, a statement that 'the number of planets' denotes the number of planets is a purely semantical statement about the English language. And indeed it would seem that a statement of this kind may be considered as purely semantical only if it is a consequence of the rules of sense, sense-range, and sense-value, together with the syntactical rules and the general principles of meaning (i)–(xvi).

Thus as derived semantical rules rather than primitive, there will be also: (8) *Rules of denotation,* by which a denotation is determined for each name. (9) *Rules of range,* assigning to each variable a range. (10) *Rules of value,* by which a value is determined for every form and for every admissible system of values of its free variables.

By stating (8), (9), and (10) as primitive rules, without (5), (6), and (7) there results what may be called the *extensional part* of the semantics of a language. The remaining *intensional part* of the semantics does not follow from the extensional part. For the sense of a name is not uniquely determined by its denotation, and thus a particular rule of denotation does not of itself have as a consequence the corresponding rule of sense.

On the other hand, because the meta-linguistic phrase which is used in the rule of denotation must itself have a sense, there is a certain sense (though not that of logical consequence) in which the rule of denotation, by being given as a primitive rule of denotation, uniquely indicates the corresponding rule of sense. Since the like is true of the rules of range and rules of value, it is permissible to say that we have fixed an *interpretation* of a given logistic system, and thus a formalized language, if we have stated only the extensional part of the semantics.[16]

Although all the foregoing account has been concerned with the case of a formalized language, I would go on to say that in my opinion there is no difference in principle between this case and that of one of the natural languages. In particular, it must not be thought that a formalized language depends for its meaning or its justification (in any sense in which a natural language does not) upon some prior natural language, say English, through some system of translation of its sentences into English — or, more plausibly, through the statement of its syntactical and semantical rules in English. For speaking in principle, and leaving all questions of practicality aside, the logician must declare it a mere historical accident that you and I learned from birth to speak English rather than a language with less irregular, and logically simpler, syntactical rules, similar to those of one of the familiar logistic systems in use today — or that we learned in school the content of conventional English grammars and dictionaries rather than a more precise statement of a system of syntactical and semantical rules of the kind which has been described in this present sketch. The difference of a formalized language from a natural language lies not in any matter of principle, but in the degree of completeness that has been attained in the laying down of explicit syntactical and semantical rules and the extent to which vaguenesses and uncertainties have been removed from them.

For this reason the English language itself may be used as a convenient though makeshift illustration of a language for which syntactical and semantical rules are to be given. Of course only a few illustrative examples of such rules can be given in brief space. And even for this it is necessary to avoid carefully the use of examples involving English constructions that raise special difficulties or show too great logical irregularities, and to evade the manifold equivocacy of English words by selecting and giving attention to just one meaning of each word mentioned. It must also not be asked whether the rules given as examples are among the "true" rules of the English language or are "really" a part of what is implied in an understanding of English; for the laying down of rules for a natural language, because of the need to fill gaps and to decide doubtful points, is as much a process of legislation as of reporting.

With these understandings, and with no attempt made to distinguish between primitive and derived rules, following are some examples of syntactical and semantical rules of English according to the program which has been outlined.[17]

(1) Vocabulary: 'equals' 'five' 'four' 'if' 'is' 'nine' 'number' 'of' 'planet' 'planets' 'plus' 'round' 'the' 'then' 'the world' — besides the bare list of primitive symbols (words) there must be statements regarding their classification into categories and systematic relations among them, e.g., that 'planet' is a common noun,[18] that 'planets' is the plural of 'planet,'[19] that 'the world' is a proper noun, that 'round' is an adjective.

(2) Formation Rules: If A is the plural of a common noun, then 'the'⌢'number'⌢'of'⌢A is a singular term. A proper noun standing alone is a singular term. If A and B are singular terms, then A⌢'equals'⌢B is a sentence. If A is a singular term and B is an adjective, then A⌢'is'⌢B is a sentence.[20] If A and B are sentences, then 'if'⌢A⌢'then'⌢B is a sentence. — Here singular terms and sentences are to be understood as categories of well-formed expressions; a more complete list of formation rules would no doubt introduce many more such.

(3) Rules of Inference: Where A and B are sentences, from 'if'⌢A⌢'then'⌢B and A to infer B. Where A and B are singular terms and C is an adjective, from A⌢'equals'⌢B and B⌢'is'⌢C to infer A⌢'is'⌢C.

(4) Axioms-Theorems: 'if the world is round, then the world is round'; 'four plus five equals nine.'

(5) Rules of Sense: 'round' expresses the property of roundness. 'the world' expresses the (individual) concept of the world. 'the world is round' expresses the proposition that the world is round.

(8) Rules of Denotation: 'round' denotes the class of round things. 'the world' denotes the world. 'the world is round' denotes the truth-value thereof that the world is round.[21]

On a Fregean theory of meaning, rules of truth in Tarski's form — e.g., " 'the world is round' is true if and only if the world is round" — follow from the rules of denotation for sentences. For that a sentence is true is taken to be the same as that it denotes truth.

NOTES

[1] For convenience of the present brief exposition we make the simplifying assumption that sentences are without free variables, and that only sentences are asserted.

[2] The possibility that the meaningful expressions may be a proper subclass of the well-formed expressions must not ultimately be excluded. But again for the present sketch it will be convenient to treat the two classes as identical—the simplest and most usual case. Compare, however, footnote 13.

[3] Such notations can be reduced to at most two, namely the notation (consisting, say, of juxtaposition between parentheses) which is used in application of a singulary function to its argument, and the abstraction operator λ. By the methods of the Schönfinkel-Curry combinatory logic it may even be possible further to eliminate the abstraction operator, and along with it the use of variables altogether. But this final reduction is not contemplated here—nor even necessarily the simpler reduction to two syncategorematic notations.

[4] The complicating possibility is here ignored of *denotationless names*, or names which have a sense but no denotation. For though it may be held that these do occur in the natural languages, it is possible, as Frege showed, to construct a formalized language in such a way as to avoid them.

[5] This is meant only as a preliminary rough description. In logical order, the notion of a concept must be postulated and that of a possible language defined by means of it.

[6] Frege's term in German is *Marke*.—The form or *Marke* must of course not be confused with its associated abstract entity, the *function*. The function differs from the form in that it is not a linguistic entity, and belongs to no particular language. Indeed the same function may be associated with different forms; and if there is more than one free variable the same form may have several associated functions. But in some languages it is possible from the form to construct a name (or names) of the associated function (or functions) by means of an abstraction operator.

[7] The idea of allowing variables of different ranges is not Fregean, except in the case of functions in Frege's sense (i.e., as *ungesättigt*), the different categories of which appear as ranges for different variables. The intro-

duction of *Gegenstandsbuchstaben* with restricted ranges is one of the modifications here advocated in Frege's theory.

[8] Exceptions to this are familiar in common mathematical notation. E.g. the form x/y has no value for the system of values 0,0 of x, y. However, the semantics of a language is much simplified if a value is assigned to a form for every system of values of the free variables which are admissible in the sense that each value belongs to the range of the corresponding variable. And for purposes of the present exposition we assume that this has been done. (Compare footnote 4.)

[9] Even if the language contains no constant denoting the value in question, it is possible to consider an extension of the language obtained by adjoining such a constant.

[10] The notion of a sense-value of a form is not introduced by Frege, at least not explicitly, but it can be argued that it is necessarily implicit in his theory. For Frege's question, "How can a=b if true ever differ in meaning from a=a?" can be asked as well for forms a and b as for constants, and leads to the distinction of value and sense-value of a form just as it does to the distinction of denotation and sense of a constant. Even in a language like that of *Principia Mathematica*, having no forms other than propositional forms, a parallel argument can be used to show that from the equivalence of two propositional forms A and B the identity in meaning of A and B in all respects is not to be inferred. For otherwise how could A $\equiv$ B if true (i.e., true for all values of the variables) ever differ in meaning from A $\equiv$ A?

[11] For purposes of the preliminary sketch, the meta-language is left unformalized, and such questions are ignored as whether the meta-language shall conform to the theory of types or to some alternative such as transfinite type theory or axiomatic set theory. Because of the extreme generality which is attempted in laying down these principles, it is clear that there may be some difficulty in rendering them precise (in their full attempted generality) by restatement in a formalized meta-language. But it should be possible to state the semantical rules of a particular object language so as to conform, so that the principles are clarified to this extent by illustration.

It is not meant that the list of principles is necessarily complete or in final form, but rather a tentative list is here proposed for study and possible amendment. Moreover it is not meant that it may not be possible to formulate a language not conforming to the principles, but only that a satisfactory general theory may result by making conformity to these principles a part of the definition of a formalized language (compare footnote 12).

[12] In the case of some logistic systems which have been proposed (e.g., by Hilbert and Bernays), if semantical rules are to be added, in conformity with the theory here described and with the informally intended interpretation of the system, it is found to be impossible to satisfy (viii), (x), and (xiv), because of restriction imposed on the bound variables which may appear in a constant or form used in a particular context. But it would

seem that modifications in the logistic system necessary to remove the restriction may reasonably be considered non-essential, and that in this sense (viii), (x), (xiv) may still be maintained.

In regard to all of the principles it should be understood that non-essential modifications in existing logistic systems may be required to make them conform. In particular the principles have been formulated in a way which does not contemplate the distinction in typographical style between free and bound variables that appears in systems of Frege and of Hilbert-Bernays.

In (x) and (xi), the condition that f′ have the same free variables as f can in many cases be weakened to the condition that every free variable of f′ occur also as a free variable of f.

[13] Possibly (ix) and (xi) should be weakened to require only that if C′ is well-formed then it is a constant having the same denotation as C. Since there is in general no syntactical criterion by which to ascertain whether two constants c and c′ have the same denotation, or whether two forms have always the same values, there is the possibility that the stronger forms of (ix) and (xi) might lead to difficulty in some cases. However, (ix) as here stated has the effect of preserving fully the rule of substitutivity of equality—where the equality sign is so interpreted that $[c_1 = c_2]$ is a sentence denoting truth if and only if c_1 and c_2 are constants having the same denotation—and if in some formalized languages, (ix) and (xi) should prove to be inconsistent with the requirement that every well-formed expression be meaningful (footnote 2), it may be preferable to abandon the latter. Indeed the preservation of the rule of substitutivity of equality may be regarded as an important advantage of a Fregean theory of meaning over some of the alternatives that suggest themselves.

[14] At the present stage it cannot be said with assurance that a modification of Frege's theory will ultimately prove to be the best or the simplest. Alternative theories demanding study are: the theory of Russell, which relies on the elimination of names by contextual definition to an extent sufficient to render the distinction of sense and denotation unnecessary; the modification of Russell's theory, briefly suggested by Smullyan (in *The Journal of Symbolic Logic*, vol. 13 (1948), pp. 31-37), according to which descriptive phrases are to be considered as actually contained in the logistic system rather than being (in the phrase of Whitehead and Russell) ''mere typographical conveniences,'' but are to differ from names in that they retain their need for scope indicators; and finally, the theory of Carnap's *Meaning and Necessity*.

Though the Russell theory has an element of simplicity in avoiding the distinction of two kinds of meaning, it leads to complications of its own of a different sort, in connection with the matter of scope of descriptions. The same should be said of Smullyan's proposed modification of the theory. And the distinctions of scope become especially important in modal statements, where they cannot be eliminated by the convention of always taking the minimum scope, as Smullyan has shown (loc. cit.).

Moreover, in its present form it would seem that the Russell theory requires some supplementation. For example, 'I am thinking of Pegasus,' 'Ponce de Leon searched for the fountain of youth,' 'Barbara Villiers was less chaste than Diana' cannot be analyzed as '$(Ec)[x$ is a Pegasus $\underset{x}{\equiv}$ $x=c]$ [I am thinking of c],' '$(Ec)[x$ is a fountain of youth $\underset{x}{\equiv}$ $x=c]$ [Ponce de Leon searched for c],' '$(Ec)[x$ is a Diana $\underset{x}{\equiv}$ $x=c]$ [Barbara Villiers was less chaste than c]' respectively—if only because of the (probable or possible) difference of truth-value between the given statements and their proposed analyses. On a Fregean theory of meaning the given statements might be analyzed as being about the individual concepts of Pegasus, of the fountain of youth, and of Diana rather than about some certain winged horse, some certain fountain, and some certain goddess. For the Russell theory it might be suggested to analyze them as being about the property of being a Pegasus, the property of being a fountain of youth, and the property of being a Diana. This analysis in terms of properties would also be possible on a Fregean theory, though perhaps slightly less natural. On a theory of the Russell type the difficulty arises that names of properties seem to be required, and on pain of readmitting Frege's puzzle about equality (which leads to the distinction of sense and denotation in connection with names of any kind), such names of properties either must be analyzed away by contextual definition—it is not clear how—or must be so severely restricted that two names of the same property cannot occur unless trivially synonymous.

[15] Here a warning is necessary against spurious economies, since not every subtraction from the entities which a theory assumes is a reduction in the variety of entities.

For example, in the simple theory of types it is well known that the individuals may be dispensed with if classes and relations of all types are retained; or one may abandon also classes and relations of the lowest type, retaining only those of higher type. In fact any finite number of levels at the bottom of the hierarchy of types may be deleted. But this is no reduction in the variety of entities, because the truncated hierarchy of types, by appropriate deletions of entities in each type, can be made isomorphic to the original hierarchy—and indeed the continued adequacy of the truncated hierarchy to the original purposes depends on this isomorphism.

Similarly the idea may suggest itself to admit the distinction of sense and denotation at the nth level and above in the hierarchy of types, but below the nth level to deny this distinction and to adopt instead Russell's device of contextual elimination of names. The entities assumed would thus include only the usual extensional entities below the nth level, but at the nth level and above they would include also concepts, concepts of concepts, and so on. However, this is no reduction in the variety of entities assumed, as compared to the theory which assumes at all levels in the hierarchy of types not only the extensional entities but also concepts of them, concepts of concepts of them, and so on. For the entities assumed by the former theory are reduced again to isomorphism with those assumed by the latter, if all entities below the *n*th level are deleted and appropriate deletions are made in every type at the *n*th level and above.

Some one may object that the notion of isomorphism is irrelevant which is here introduced, and insist that any subtraction from the entities assumed by a theory must be considered a simplification. But to such objector I would reply that his proposal leads (in the cases just named, and others) to perpetual oscillation between two theories T_1 and T_2, T_1 being reduced to T_2 and T_2 to T_1 by successive "simplifications" *ad infinitum.*

[16] As is done in the forthcoming revised edition of my *Introduction to Mathematical Logic, Part I.*

[17] For convenience, English is used also as the meta-language, although this gives a false appearance of triviality or obviousness to some of the semantical rules. Since the purpose is only illustrative, the danger of semantical antinomies is ignored.

[18] For present illustrative purposes the question may be avoided whether common nouns in English, in the singular, shall be considered to be variables (e.g., 'planet' or 'a planet' as a variable having planets as its range), or to be class names (e.g., 'planet' as a proper name of the class of planets), or to have "no status at all in a logical grammar" (see Quine's *Methods of Logic,* p. 207), or perhaps to vary from one of these uses to another according to context.

[19] Or possibly 'planet' and 's' could be regarded as two primitive symbols, by making a minor change in existing English so that all common nouns form the plural by adding 's.'

[20] If any of you finds unacceptable the conclusion that therefore 'the number of planets is round' is a sentence, he may try to alter the rules to suit, perhaps by distinguishing different types of terms. This is an example of a doubtful point, on the decision of which there may well be differences of opinion. The advocate of a set-theoretic language may decide one way and the advocate of type theory another, but it is hard to say that either decision is the "true" decision for the English language as it is.

[21] But of course it would be wrong to include as a rule of denotation: 'the world is round' denotes truth. For this depends on a fact of geography extraneous to semantics (namely that the world is round).

Proceedings of the American Academy of Arts and Sciences

Vol. 80, No. 2, P. 113-186, May, 1952

CONTRIBUTIONS TO THE ANALYSIS AND SYNTHESIS OF KNOWLEDGE

2

Published in cooperation with the
Institute for the Unity of Science

CONTENTS

The Origin of the Separation between Science and Philosophy — PHILIPP FRANK 115

Brief Bibliography for the Sociology of Science
BERNARD BARBER and ROBERT K. MERTON 140

Brief Bibliography of Formal Logic
ALONZO CHURCH 155

Some Significant Trends toward Integration in the Sciences of Man — LAURA THOMPSON 173

THE ORIGIN OF THE SEPARATION BETWEEN SCIENCE AND PHILOSOPHY

PHILIPP FRANK

1. DE BROGLIE AND EINSTEIN ON SCIENCE AND PHILOSOPHY

When it comes to the question of science and philosophy, the average college student and even the average college professor are isolationists. Physical science is one department, and philosophy is another. They appear to have little in common. If one were to say that in order really to understand his own field the physicist or biologist needs to know something about philosophy and the philosopher needs to know something about science, most people would be quite surprised and probably somewhat incredulous. And yet the isolationist attitude on this subject so common among our college population, students and faculty alike, is not shared by the most creative scientific minds of our age. Certainly it was not the attitude of past generations: for centuries the unity of philosophy and science was taken for granted. But a belief in their unity is not the exclusive property of the past. The men who have laid the two cornerstones of physical science in the twentieth century — the wave theory of matter and the theory of relativity — can testify instead.

Prince Louis de Broglie, author of the wave theory of matter, writes in his book, *The Future of Physics*:

> In the nineteenth century, there came into being a divorce between scientists and philosophers. The scientists looked with a certain suspicion upon philosophic speculations which appeared to them too frequently to lack precise formulation and to attack vain, insoluble problems. The philosophers, in turn, were no longer interested in the special sciences because their results seemed too narrow. This separation, however, has been harmful to both philosophers and scientists.

Albert Einstein, author of the theory of relativity, writes:

> I can say with certainty that the ablest students whom I met as a teacher were deeply interested in the theory of knowledge. I mean by "ablest students" those who

> excelled not only in skill but in independence of judgment. They liked to start discussions about the aims and methods of science, and proved by their obstinacy in the defense of their opinions that this issue was an important one to them.

Niels Bohr, to whom we owe our present views about the structure of the atom, might easily be quoted in the same vein. It is he who advanced the basic principle of subatomic physics — the principle of complementarity.

In spite of what these three great men hold to be true regarding the interdependence of science and philosophy, in actual practice the gap remains. The two fields are taught separately, and research in them is pursued separately. We mentioned before that this has not always been the case. Initially, science and philosophy were one, and they drifted apart only by a gradual process. Let us see whether we can follow this process:

2. Science and Technology

Everyone who is at all familiar with science knows that science provides us with information about how to repair a radio set, how to grow corn, how to produce atom bombs, etc. But this kind of information does not exhaust what we can learn from science. For example: something is wrong with your radio, and there are two people whom you could ask to repair it: one is the janitor, who happens to know a good deal about your particular make of radio; the other is a physics professor whose particular specialty has almost nothing to do with radios. If you want the job done quickly you will ask the janitor — he has the technical "know-how". On the other hand, the professor knows something which the janitor does not know, namely, the fundamentals of electromagnetism. Hence, if you allow him sufficient time he will find out how to apply his knowledge of electromagnetism to the repair of your radio set. Not only that, but he will then be able to take care of all types of radio sets, and all sorts of other electrical devices as well, such as short-wave generators, cyclotrons, Geiger counters, etc. The janitor in this case is a technologist; the professor is a scientist.

The British mathematician W. K. Clifford made a very lucid and precise distinction between science and technology in one of his lectures:

> Now it seems to me that the difference between scientific and merely technical thought is this: Both make use of experiment and direct human action; but while technical thought or skill enables a man to deal with the same circumstances that he has met with before, scientific thought enables him to deal with different circumstances that he has never met before. . . . The aim of scientific thought is to apply past experience to new circumstances.

The question now arises: what instrument can we use to apply past experiences to those new circumstances, the circumstances we meet in the future? It becomes obvious that there can be no instrument whatever unless we assume that the future will in a certain manner resemble the past. Clifford says:

> The instrument is an observed uniformity in the course of events. By the use of this instrument it gives us information transcending our experience; it enables us to infer things that we have not seen from things that we have seen.

To work on the production of this instrument means to study the uniformities in nature, and this work is called "scientific research." The difference between science and technology is that science encompasses a wider range of uniformities. To technology belong the uniformities which are used to repair a special make of radio; but the uniformities which we find in all electromagnetic phenomena belong to science.

3. The Natural Path of Investigation

Anyone who has worked in science, not only to open a door to technology but also to search for the uniformities or laws of nature, has felt the urge to proceed to laws of higher and higher generality. When research is carried forward to its limits, one occasionally comes upon questions which are not the property of any of the "special" sciences, such as physics, psychology, or economics. One is led to investigate the properties of space and time, the relation between cause and effect, the reasons for uniformity in nature, the relation between our sense perceptions and underlying physical reality, the coexistence of human "freedom" and "iron" physical causality. There is some doubt as to whether all these questions belong in the domain of "bona fide" science. Without deciding this issue one way or the other, we can say for the time being that this kind of question belongs to a field known in our vague, everyday language as "philosophy."

Thus there is a path which leads from technology to science and from science to philosophy. We can describe it in simple terms by saying that it is a gradually increasing unification of our knowledge. This is the way it was described by the British philosopher, Herbert Spencer: "Knowledge of the lowest kind is ununified knowledge; science is partially unified knowledge; philosophy is completely unified knowledge."

Science, then, starts as observation and experiment, and winds up as a system of abstract concepts and principles. The Greek philosopher Aristotle gives us a good notion of how this happens:

> The natural path of investigation starts from what is more readily knowable and more evident *to us* and proceeds to what is more self-evident and intrinsically more intelligible; for it is one thing to be knowable to us and quite another thing to be intelligible objectively. This, then, is the method prescribed: to advance from what is clearer to us, though intrinsically more obscure, towards what is intrinsically clearer and more intelligible.

Before we can understand the relation between science and philosophy, we must understand this distinction which Aristotle makes between what is "more readily knowable to us" and what is "intrinsically more intelligible." When Aristotle says that something is "clear and knowable to us," he means that it is "clear to our immediate sense experience." But to Aristotle immediate sense impressions are crude and "general." It is only when they have been subjected to careful analysis that they can be reduced to their objectively simple elements or "particulars."

> Now what is plain and obvious at first are rather confused aggregates, the elements and principles of which become known to us later by analysis. Thus we must advance from generalities to particulars, for it is a whole which is best known to sense perceptions and a generality is a kind of a whole, comprehending many things within its parts.

4. "DIRECT KNOWLEDGE" AND "INTELLIGIBLE CONCEPTS"

Aristotle calls a concept "intelligible intrinsically" if it is more useful in the formulation of simple laws of nature than are crude sense impressions. We may explain this concept of "intelligibility" with an example taken from elementary mechanics. We are observing a material body, say a ball, falling to the ground from a height. At the same time, we have a clock. Our crude sense perceptions tell us that between the act of dropping the ball and the moment when it hits the ground, the hands of the clock have advanced. We might drop other objects, such as stones or apples, at different times, in different places and from different heights — yet casual observation alone would not necessarily lead us to note any uniformity in the motions of these falling bodies, except perhaps that the farther they fell the faster they fell. Galileo, however, went farther; he conducted experiments, and through his analysis of the results he was able to formulate the concepts which enable us to describe the motions of falling bodies — acceleration and resistance. With the aid of these concepts, it is possible to describe very simply the two extreme cases: in very thin air, acceleration is constant; in very dense air, resistance is so high that acceleration reaches the vanishing point. But we must go still farther; in order to define the concept of acceleration for all possible motions, we must define the concept of velocity, and in order to do this we require the concept of the differential coefficient of a function. All the concepts needed to describe motion — the instant of time, acceleration, differential coefficient, velocity — are "intelligible" to our reason and "intrinsically" clear. But to our immediate sense perception they are obviously obscure.

Let us take another example from modern physics. When an atom bomb explodes we see a cloud shaped like a mushroom — this is knowable to us by immediate sense perception. But there is another way of describing that explosion, namely Einstein's formula for the conversion of mass into energy, $E=mc^2$. The concepts Energy (E) and mass (m) are "intelligible" to our reason, but they are anything but clear to our immediate sense perception.

This contrast between the results of direct sense observation and the results of rational analysis has dominated the philosophy of science as long as any science has existed. The two concepts of "what is simple and knowable to us" and "what is obscure to us" have been the source of great intellectual struggles, and it is from the discussions centering about this contrast that modern science and its philosophical interpretation have developed. As long as men have sought knowledge, they have sought a few simple principles which would explain logically the enormous complexity of observable facts. But this search has never failed to be accompanied by great distrust of abstract knowledge — a distrust born of the fear that formulae might never exhaust the richness of the actual world.

There has always been a "scientific" type of man, who needs only to acquire a few principles from which observable facts can be logically derived in order to feel that he "understands the world." But there has also always existed the kind of man who achieves this feeling of understanding when he surrenders as passively as possible to the flow of sense impressions received from the actual world. These two approaches are described by F. S. C. Northrop as "the Western and the Eastern type of thought."

5. "SCIENCE" AND "PHILOSOPHY" WERE ONCE ONE AND THE SAME

For the ancient Greeks, every investigation which led from immediate sense experience to the abstract principles from which this experience could be derived was called "science" or "philosophy." There was not much differentiation between the meanings of these two terms. A recent French philosophical dictionary gives as its first meaning for the word "philosophy" that ancient definition: "rational knowledge, science in the most general sense of the word."

As a matter of fact, it is characteristic of all languages whose scientific vocabulary, like that of English, is based upon the Greek that there has in the past been very little distinction in meaning between "science" and "philosophy." In the writings of Aristotle, systems of knowledge about inanimate bodies (physics), animate beings (biology), and human societies (politics) were treated as part of "philosophy" or "science." The same broad meaning prevailed during the Middle Ages. A modern author whose philosophy is constructed along the lines of medieval Thomism defines philosophy as follows: "Philosophy is the scientific knowledge of all things, gained through consideration by the natural light of reason of their fundamental reasons and causes." In this definition there is a phrase which Aristotle would never have used in the same sense — "the natural light of reason." This expression, so characteristically medieval, implies that philosophy is not the only kind of knowledge, that there are other kinds which must be acquired by other means.

Even today, of course, the broader use of the word "philosophy" has not disappeared. Our universities use it all the time in awarding the degree of "Doctor of Philosophy" to students who have fulfilled their requirements in one of the special sciences.

6. "SCIENCE" AND "SACRED DOCTRINE"

The medieval philosophers used the word "science" in a sense which is more general than that of "philosophy." "Science" includes not only the "philosophical knowledge" which is "gained by human reason" but also "sacred doctrine" which can be obtained only by "divine revelation." St. Thomas Aquinas, the leading philosopher of the Middle Ages, raises directly the question whether "sacred doctrine" belongs in the realm of "science", a question which he answers in the affirmative. Man can obtain knowledge about the existence and the properties of God by the light of his own reason, says St. Thomas but

> even as regards those truths about God which human reason can investigate, it was necessary that man be taught by divine revelation. For the truth about God, such as reason can know it, would only be known by few and with the admixture of many errors.

In other words, there are a great many things about God and the world which we know by revelation, and could not know without it. Medieval writers, as well as a great many later ones, distinguished, therefore, between "natural" or "philosophical" theology on the one hand, and "sacred" theology, derived from revelation, on the other. Thomas Aquinas attempts to demonstrate that "science" comprises both "natural theology" and "sacred doctrine," but that philosophy pertains only to "natural theology." It might be said of the Middle Ages, then, that science and philosophy are not the same; rather, science *includes* philosophy.

7. MEDIEVAL ROOTS OF EMPIRICISM

In the late Middle Ages there arose a new philosophical movement which was to represent the transition from medieval to modern thought. The significant feature of this movement was that, while in principle it was based on the Aristotelian and

Thomistic patterns of thought, it gave much greater emphasis to the role of experience in science. This does not mean that there was a complete break with the older philosophers. The new movement still attributed paramount importance to ''revelation'' or ''revealed truth.'' But a new element had without doubt been introduced, as we can see from the following excerpt from the writings of Roger Bacon:

> There are two ways of acquiring knowledge: namely by argumentation and by experience. Argumentation arrives at a conclusion and makes us agree with it. But argumentation does not banish doubt so effectively that the mind rests in intuition of the truth, until the truth is discovered by way of experience. . . . If someone who has never seen fire has worked out an adequate proof that fire injures and destroys things yet the mind of the student would never be content with this demonstration, nor would he avoid contact with flames. But once the experience of combustion has been had the mind is reassured and rests content in the splendor of the truth. . . . All statements must therefore be confirmed by way of experience. But experience is twofold. One type is attained by external sense perception. Moreover, what occurs in regions other than those where we are situated we learn from reports from other savants who have experienced them. This type of experience is natural to man and belongs to philosophy.

Roger Bacon assigns to philosophy all human knowledge which man obtains from his natural reason and natural experience. Nevertheless he believed, as Thomas Aquinas did, that there was also a science transcending natural experience and reason, which was based on experience of a different sort.

Bacon stresses the point that natural experience is inadequate for man. First,

> because it does not give complete assurance about his knowledge of material things, which are difficult to understand and secondly, because it provides no evidence at all about spiritual things. . . . It is, therefore, indispensable that the human mind receive some assistance in some other way. And this is the way the holy patriarchs and prophets who first brought science to the world received interior illumination and did not rely only upon sense evidence.

The remarkable thing about Roger Bacon was that his emphasis on the importance of experience and his distrust of pure reasoning seem to have led him to greater confidence in "interior illumination" or "divine revelation." But it must be remembered that "experience" for Bacon meant not only sense experience but also "supernatural" experience — and both for him were more reliable than what he called argumentation. This correlation between a strong emphasis on personal experience and a belief in supernatural phenomena has appeared again and again in intellectual history, as we shall see later on.

8. Descartes and the "Tree of Philosophy"

The period around 1600 marked the "birth of modern science and philosophy," and just as antiquity had its Aristotle and the Middle Ages its Thomas Aquinas, so René Descartes was the writer who formulated for his time the general principles of the new science. In the preface to his *Principles of Philosophy*, the French philosopher and scientist used the famous image of the tree to illustrate his ideas about the structure of science and philosophy.

> All philosophy is like a tree, of which metaphysics is the root, physics the trunk and all the other sciences the branches that grow out of the trunk. They are reduced to three principal ones, namely: medicine, mechanics, and ethics.

The image of the Cartesian tree may be translated into more familiar terms: the roots (metaphysics) are the most general philosophical principles which originate in introspection; they deal with the reality of the external world, space and time, etc. The trunk (physics) comprises the laws governing the physical world, such as the conservation of mass, momentum, and energy, the laws of motion, etc., while the branches are applied science: the practical applications of general scientific principles to mechanical engineering, to the art of healing, and to the rules of human behavior. Descartes continues:

> As it is not from the roots or trunks of trees that we gather the fruit but only from the extremities of the branches, so the principal utility of philosophy depends on the separate uses of its parts which we can only learn last of all.

It is important to note that Descartes did not ascribe any utility to general principles alone; they had to be checked through their observable consequences or "fruits." These "fruits" of the tree are the technological applications of scientific knowledge to human labor, human health and human conduct. Thus the Cartesian tree embraces "science" or "philosophy" in the ancient sense; it contains all the stages of unified knowledge, from the practice of everyday life to the abstract principles of metaphysics.

9. The Empirical Knowledge of the Craftsman

Attempts were undoubtedly made in antiquity and in the Middle Ages to draw conclusions from the generally accepted principles of philosophy which might make human life more comfortable and pleasant. Whatever theoretical principles were espoused concerning the nature of inanimate bodies ought to have furnished a few practical principles which could be used in building and engineering. Theories about the nature of animate beings ought to have led to some helpful contributions to the art of medicine. But actually, with all the principles announced during all these centuries, there was very little that was logically cogent or practically useful derived from them. Only in the field of astronomy, and to a lesser degree in the field of medicine, did there exist a certain body of knowledge which was at the same time practically useful and based on philosophical principles. But even in these fields, the bond between principle and fact was hardly strong, as we shall see when we try to understand the Copernican revolution in astronomy. Suffice it to say that before 1600, before the "birth of modern science and philosophy," the crop of fruits from the tree of philosophy was a fairly meager one.

On the other hand, the ancient Greeks and Romans and the ancient Egyptians displayed a marvelous art and skill in building and even in mechanical engineering. Their vaults and arches which bore such heavy loads were constructed with such precision and strength that they indicate a highly sophisticated knowledge of practical mechanics. Be this as it may, the knowledge of these ancient builders and engineers was not scientific but technological. It was purely empirical, not based upon philosophical principles. The contrast between the modern and the ancient approach to technical knowledge is described by a professor at the school of applied engineering in contemporary Rome:

> What modern science and industry accomplish by laboratory research tests, theoretical hypotheses expressed in formulae . . . was accomplished for the science and industry of ancient times by the transmission of technical knowledge . . . and by empirical formulae, jealously guarded and handed down in mysterious symbolic form.

10. Practical Mechanics in Ancient Rome

One of the very few books on technology which has been preserved from ancient Rome is Vitruvius' book on architecture, a work which gives us a fairly clear idea of technical "know-how" among the Romans. Vitruvius discusses the mechanical devices which were used in building to hoist heavy loads, giving the most precise directions on how to construct systems of axles and wheels, pulleys and levers, in order to obtain a machine which would save labor. He defines a machine as "a continuous material system having special fitness for the moving of weights." He also describes machines which launched projectiles to be used for military purposes. With the greatest accuracy and with the occasional use of mathematical calculation, he offers detailed descriptions of complicated systems of pulleys and levers. Still, the labor saving effect of these machines is an empirical matter, a matter of experience, and there is hardly any attempt made to derive concrete results from general physical laws. Every once in a while, however, Vitruvius hints that behind the operations of these mechanical devices, there may be universal laws of nature.

> All machinery, he says, is generated by Nature and the revolution of the Universe guides and controls. Since our fathers have observed the revolutions of the sun and the planets they took precedents from Nature, imitating them, and led on by what is divine they developed the comfort of life by their inventions. And so, they rendered things more convenient by machines and their revolutions.

Vitruvius says of the lever, "The short end of the lever is placed under the load, and the long end of the lever, when it is pressed down by one man's strength, raises the load." He understood how a lever could be used to lift loads which were too heavy for the strength of one man. And yet this understanding is based not on the laws of equilibrium on the lever, which had been established before his time by Archimedes, but rather on his own intuitive insights acquired through practice. He attempts a crude explanation: "When the lever is in action the circular motion of the lever round the fulcrum causes the weight of a great load to be balanced by few hands." This "rule of thumb," this expression of what the Roman engineer *felt* to be true, is probably what later developed into the "principle of virtual displacements." But apparently the greatest extent to which the Romans related the power of the lever to the rest of nature was in their "feeling" that the use of rotating bodies was a kind of imitation of the revolution of the planets.

11. THE LOW SOCIAL STATUS OF EMPIRICAL INVESTIGATION IN ANCIENT GREECE

The wide gap which existed throughout antiquity and the Middle Ages between "principles" and "know-how," between "science" and "technology," was intimately connected with the social structure. The "know-how" of the craftsman and the engineer was acquired through physical work, not through pure intellectual effort. It was the empirical knowledge of the man of low social status, the artisan and even the slave. Philosophical knowledge was cultivated and promoted by men of high social status, in particular priests and statesmen. The "lower" strata collected "facts"; the higher-ups advanced principles. Contact between the two types of knowledge was discouraged by social custom. If a man of high social status attempted to apply his "philosophy" or "science" to the work of the craftsman, he was ostracized.

A case in point is that of the great Greek mathematician Archimedes, whose name is known to every student of high school physics who has learned "Archimedes' principle" of hydrodynamics. Another contribution of his, mentioned above, was the law of equilibrium on the lever, which he derived from what he believed to be self-evident principles. But he did not restrict himself to this "philosophical" or "scientific" work; he saw that his "law of the lever" had applications, and he applied it to the construction of machines which could lift heavy loads by the exertion of a small amount of force. During the siege of his native city of Syracuse, Archimedes acted as scientific adviser to the armed forces of Syracuse, for he furnished directions for the building of machines which could hoist the enemy fighting vessels right out of the water and render them ineffectual. Something extraordinary had taken place: "pure science" had been put in the service of practical warfare.

The scientists and philosophers disapproved. This putting of scientific abilities to practical use meant a degradation of standards. The Greek biographer Plutarch writes about Archimedes: "He did not think the invention of engines for military purposes an object worthy of his serious studies, but only reckoned them among the amusements of geometry." For Plutarch, Archimedes was like those Greek mathematicians whose chief concern was the demonstration of geometrical theorems, but who happened to employ mechanical instruments of measurement to do so, instead of pure logical deduction. This view of philosophy and its relation to the social order had been crystallized by Plato, who placed philosophy high above empirical knowledge and technical application; he took strong exception to scientists who attempted to confirm their theorems by testing their consequences in the external world through sense observation. "Plato inveighed against them with great indignation," writes Plutarch, "as corrupting and debasing the excellency of geometry, by making her descend from incorporeal and intellectual to corporeal and sensible things."

The inferior status of technology or technical application mirrored the inferior social status of the working man in Greek society. Plutarch attributes to Plato the statement that who applies mechanical instruments in geometry has "to make use of matter, which requires much manual labor and is the object of servile trade."

When mechanics as a system of knowledge became connected with technical application it was, according to Plutarch, "separated from geometry and being a long time despised by the philosophers was considered as a brand of military art."

When we compare the modern period with antiquity and the Middle Ages, we see that each era had its barriers, its "gaps." But whereas today there is a gap between science and philosophy, in ancient and medieval times it was science and philosophy on one side arrayed against empirical or technical knowledge on the other.

12. How "Modern Science" Was Born

One of the greatest of modern philosophers, A. N. Whitehead, writes:

> All the world over and at all times there have been practical men, absorbed in irreducible and stubborn

> facts; all the world over and at all times there have been men of philosophic temperament, who have been absorbed in the weaving of general principles.

During ancient and medieval times these two kinds of men had little to do with each other. Whitehead holds that the "birth of modern science" is closely connected with the beginnings of cooperation between them.

> The union of passionate interest in the detailed facts with equal devotion to abstract generalization forms the novelty in our present society. Previously it had appeared sporadically and as if by chance.

Archimedes' application of mathematics to engineering was the kind of sporadic event Whitehead refers to. In our society, this "balance of mind" has now become an essential factor in our cultural tradition. "It is," says Whitehead, "the salt which keeps life sweet." Following from this, Whitehead's understanding of the part education ought to play in the modern world is expressed clearly and wisely: the role of the university is "to transmit the interest in the relation of general principles to stubborn facts from generation to generation."

Around 1600, there was a shifting of class structure; the distance between the craftsman and the statesman was narrowed. The breaking down of social barriers was accompanied by an increasing closeness between interest in principles and interest in facts. There was a new balance of mind, a new science which undertook to establish general principles which would serve the needs of practical mechanics and medicine. The birth of modern science meant the development of the law of inertia and the law of gravity, and from these and other principles the men of the new era were able to leave behind the "rule of thumb" and to derive logically the practical steps to be taken in dealing with "stubborn facts." Not only could they hoist heavy loads, but they could introduce precision into many new activities from the making of clocks to the aiming of cannon balls.

13. The Beginning of the Split Between Science and Philosophy

"Philosophy" or "science" in its original broad sense had now become an endeavor of extreme complexity. It had to do more than set up principles which were "intrinsically intelligible"; these principles had to be such that results could be derived from them which would aid the engineer in building domes and bridges. Little wonder that cracks and breaks developed in the long chain of propositions needed to connect the philosopher's principles with the craftsman's "know-how." The first conspicuous break occurred in astronomy. Actually, this was to be expected, for even in antiquity astronomy had touched both philosophical principle and empirical fact. There was an intelligible theory about the stars in the sphere, but ancient astronomers were also able to derive the positions of the stars and to check them by observations.

When the demands of modern science forced stricter agreement between principle and fact, however, astronomers were faced with a dilemma. Which is more important to require: that the principles from which we start to be "intelligible" and "intrinsically knowable," or that the results of these principles agree with the facts as they are observed? For it was becoming more and more obvious that both requirements could not be met at once.

The alternative had already been explicitly formulated by the ancient Greeks. They accepted without question the hypothesis that celestial bodies move in concentric circular orbits around the earth, while the earth itself remains stationary at the center of the universe. This principle was "intelligible" because the circle was regarded as the most perfect curve and celestial bodies as the most perfect beings, unchangeable and eternal. Therefore, their movement, in order to be consistent with their nature, had to follow (the philosophers thought) the perfect and unchanging circular orbit. But the astronomers of Plato's time knew very well that accurate observation of the sun and the planets and their positions was not consistent with the hypothesis of the concentric circles.

Two instances of this inconsistency are the position of the sun and that of the planets. Let us assume that the sun actually moves around the earth along a perfectly circular orbit at constant speed. In that case, its position in the sky as we see it ought to advance each day by an equal distance along the sphere, in other words, by an equal angle. But the Greeks knew for a fact that this angle really varies at different seasons of the year. The hypothesis does not gibe with the facts. The planet Jupiter completes an approximate circle every twelve years, but study reveals that it describes a small loop along the circle every year; this means that Jupiter has a period of "retrograde motion" twelve times a year. These "irregularities" in the positions of the sun and the planets are in conflict with the principle of concentric circles.

14. ASTRONOMICAL HYPOTHESES AND "INTELLIGIBLE" PRINCIPLES

In an effort to bring principles and facts closer together, the Greek astronomers suggested two modifications in the theory of concentric orbits: the theory of eccentrics and the theory of epicycles. According to the first, the earth is not situated exactly in the center of the sun's circular orbit. Under these circumstances, if the sun moves with constant speed, the radius of vision from earth to sun will not be expected to shift with equal angular speed during the year, and the irregularities in the observed motion of the sun on the sphere can be accounted for. The theory of epicycles, on the other hand, assumed that what is moving on a circular orbit around the earth is not the real sun but a "fictitious sun" or "auxiliary geometrical point." The real sun moves along an auxiliary circle, or "epicycle," the center of which is the fictitious sun. The constant speeds of the real and fictitious suns differ, and thus when the motion of one is superimposed on that of the other the result is a single circular motion which varies in speed. This theory of epicycles also accounted for the loops in planetary cycles. It was supposed that while a fictitious Jupiter revolved around the sun in twelve years, the real Jupiter revolved around the fictitious one in a single year. These two motions taken together result in the actual twelve-year cycle with its yearly loops.

The principle that all celestial bodies move in concentric circular orbits around the earth was "intelligible" because it could be derived from the nature of celestial bodies and geometrical figures. The hypotheses of eccentrics and epicycles were not "intelligible" but they accounted for the observed positions of the sun and the planets on the sphere. The choice before the astronomer and the philosopher seemed to be either (1) to stick to "intelligible" principles and resign oneself to their lack of agreement with observed facts or (2) to give up hope of understanding the world by reason alone and be satisfied with "unintelligible" hypotheses which agreed with the facts. For the hypothesis of epicycles was certainly far from "intelligible."

15. OBSERVATIONAL AND "TRUE" ASTRONOMY

This situation produced a break in the frail chain that joined intelligible principles with observable facts. The universal "science" which was also called "philosophy" split wide open, and the task of "science" was now to think up principles from which observable facts could be derived as precisely as possible, whether those principles were intelligible or not. In this new sense, the theory of epicycles was "good science."

Plato distinguished very carefully between the two kinds of astronomy and placed them in a definite order of value. In the *Republic*, Plato stresses the point that the observed motions of celestial bodies on the sphere are complicated, disorderly and without beauty. To record them exactly as the observational astronomer does is to perform the lowest function in this branch of knowledge. But the human mind can resolve the disorderly into a sum of simple and regular motions. The movements of the sun on the sphere have been accurately observed for centuries. It is common knowledge that the sun describes an approximate circle on the sphere every day, and that its movements change slowly from day to day. The sum total of these gradually changing movements of the sun is a spiral which is the sum of two circular motions; one daily motion along the equator, and one annual motion along the ecliptic (Zodiac). Plato regarded the spiral as lacking in beauty. But the circular motions were real and beautiful to him. They were derived from a mathematical analysis of the spiral; they were the product of reason, and as such of a higher order of reality than the spiral, which was a product of mere sense perception.

But Plato did not stop there. He believed that the human mind could go farther in its inquiry and seek the causes of these "real" motions. Plato found them in the divine nature of celestial bodies, whose perfect wisdom produced perfect orbits. Thus from mathematical analysis of "observed orbits" came "true astronomy," the system of perfect orbits.

16. THREE DEGREES OF KNOWLEDGE IN ASTRONOMY

The French scientist, philosopher and historian Pierre Duhem characterizes Plato's ideas on the science of astronomy very aptly:

> There are three degrees of knowledge. The lowest degree is knowledge by sense observation. . . . The supreme degree is knowledge by pure intellect; it contemplates eternal beings and, above all, the sovereign good. Between the first (lowest) and the third (supreme) degree of knowledge is a kind of mixed and hybrid reasoning which occupies the intermediate (second) degree. The knowledge born of this intermediate reasoning is geometrical knowledge. To these

> three degrees of knowledge correspond three degrees of astronomy.
>
> Sense perception is responsible for the astronomy of observation. This kind of astronomy pursues the complicated curves described by the stars . . . and cannot provide any commensurable relation to the arithmetician, nor any definite figure to the geometrician. . . . Through geometric reasoning the mind produces an astronomy which is capable of precise figures and constant relations. This "true astronomy" replaces the erratic paths which observational astronomy attributed to the stars by simple and constant orbits; it produces the complicated and variable appearances which are false knowledge. . . . It approaches knowledge by pure intellect, which reveals the third and supreme astronomy, theological astronomy. . . . In the constancy of celestial motions it sees a proof of the existence of divine spirits which are united with the celestial bodies.

According to Duhem, in short, Plato distinguishes three degrees of astronomy: observational, geometrical, and theological.

The Platonic conception of astronomy makes clear the nature of the split within "philosophy" or "science." Observational and geometrical astronomy together constitute an instrument which can predict the observable positions of the stars, but "theological astronomy" is not necessary or even helpful for this purpose. What it *can* do is to give us a picture of the universe which will be considered a replica of the ideal human society; it can help shape human conduct.

17. "Philosophy" in the Ancient Sense Breaks Up

In order thoroughly to understand the split in philosophy, we must realize that it took place within "geometrical astronomy" itself. The perfection of circular motions could be derived from the divinity of the stars, but the fact that these motions had to be superimposed upon each other in order to produce agreement with sense observations was a human, not a divine, matter. One of Aristotle's commentators, the Greek writer Simplicius, puts the situation this way:

> Plato assumes the principle that the celestial bodies move in a circular motion, uniformly and regularly. He puts to the mathematicians the following problem: What

> circular and regular motions have we to assume as hypotheses in order to save the appearances presented by the motion of the planets?

This is a neat statement of the purpose of such hypotheses as those of the eccentrics and the epicycles; they "save appearances." They do not purport to give us further insight into the nature of the universe nor any example for moral conduct.

A division has taken place. On one side we observe celestial bodies, and employ geometrical figures to support those observations — we save appearances; on the other, we interpret those same geometrical figures as effects of perfect beings. The first approach may be called "astronomy proper," the second the "philosophical interpretation of astronomy."

Mechanics, physics, chemistry, in fact all branches of knowledge underwent a similar division as they developed to the point where observable facts could be derived from abstract principles through logic of mathematics. More and more stress was laid on the necessity for allowing any kind of hypothesis, just so long as it fitted the facts. The slogan was, "save appearances"; science in the modern sense, or "science proper," left "science in the ancient sense" behind, and came into its own.

But the split left a task to be performed: to derive these principles which "saved appearances" from others which revealed the nature of the universe, and might serve as a guide to human conduct. This task became the property of a special field of knowledge, that of "philosophy proper" as opposed to "science proper." The distinction between the two became incorporated into academic life; colleges have a "department" of philosophy and "departments" of science. The former belongs to the area of the humanities and is quite separate and distinct from the area of natural science.

18. THE "PHILOSOPHICAL" AND THE "SCIENTIFIC" CRITERION OF TRUTH

"Science proper" evolved slowly in the Middle Ages, and not much attention was paid to it during that period. But the greatest of the medieval philosophers, Thomas Aquinas, understood the division clearly and was even able to trace its origin to astronomy. St. Thomas writes:

> Reason may be employed in two ways to establish a point: firstly, for the purpose of furnishing sufficient

> proof of some principle as in natural science, when sufficient proof can be brought to show that the movement of the heavens is always of uniform velocity. Reason is employed in another way, not as furnishing a sufficient proof of a principle, but as confirming an already established principle by showing the congruity of its results, as in astrology the theories of eccentrics and epicycles is considered as established because thereby the sensible appearances of the heavenly movements can be explained; not, however, as if the proof were sufficient, for as much as some other theory may explain them.

In contemporary terms, St. Thomas is saying that there are two criteria by which the truth of a scientific statement can be checked: (1) it can be derived logically from self-evident statements and (2) it can be treated as a hypothesis from which we can derive consequences which are then checked with the facts. An example of the first check is the statement that heavenly bodies move in circles at constant speed, which is derived from the statement that heavenly bodies are the most perfect beings and can therefore move only in the most perfect way. The second criterion holds if the consequences of the hypothesis actually check with, say, the evidence of direct sense observation. But this is not a real "demonstration," because the same consequences might well have been deduced from different hypotheses. Using terms which are by now familiar to us, we may call the first criterion "philosophical" and the second "scientific." To Thomas Aquinas, a statement was never unambiguously "demonstrated" by "science proper," only by "philosophy proper," which alone provides the necessary degree of certainty.

19. SCIENCE AND PHILOSOPHY IN NEWTON'S THOUGHT

The distinction between these two criteria of scientific truth has survived from the time of St. Thomas to the present day, although in weakened form. The modern physicist would not be satisfied with a statement whose sole virtue lies in the fact that many observable facts can be derived from it. He will also require that the statement be derivable from a system of principles in which he believes more strongly than in any single fact. The scientist of today would certainly not believe that general principles like the conservation of mass, momentum or energy are self-evident, but he would consider them highly plausible. Even if isolated facts turned up which contradicted these principles, he would not abandon them until he was able to replace them with others of equal generality. In the light of this, St. Thomas' "two criteria" of truth and Aristotle's distinction between the "directly knowable" and the "intrinsically intelligible" prove to be helpful as a frame of reference in our study of the relations between philosophy and science throughout history.

When Galileo and Newton advanced the general principles of "modern science" (the principle of inertia, the law of forces, the law of gravitation, etc.), students of science learned how to hoist heavy loads, aim cannon balls and predict the paths of the planets and comets by deriving methods for doing so from these laws. But one question remained unanswered: are these laws "intelligible"? Do they fulfill the "philosophical" criterion of truth as Aquinas meant it? The "scientist proper" was now satisfied if observable facts could be derived from these laws. But the "philosopher proper" was not satisfied; he demanded "demonstrations," that is, derivation from intelligible principles. Newton himself was well aware of this situation. He realized that the philosophers objected to his theory of gravitation because it "explained" neither gravity nor inertia. In a letter addressed to Leibnitz in a Journal, Newton writes:

> To understand the motions of the planets under the influence of gravity without knowing the cause of gravity is as good a progress in philosophy as to understand the frame of a clock and the dependence of the wheels upon one another without knowing the cause of the gravity of the weight which moves the machine is in the philosophy of the clockwork.

Newton regarded his theory of gravitation as analogous to the description of a clockwork that keeps the planets moving. He agreed that the derivation of the theory from a more general and intelligible principle would contribute to philosophical progress; but as far as "modern science" was concerned, Newton claimed that progress had been made if observable motions could be derived from a law, even though the law were not "intelligible." This is what he meant by his famous statement, "I don't invent hypotheses." He preferred less intelligible laws which agreed with the facts to highsounding hypotheses which did nothing much to "save appearances."

In Newton's time, however, the break between science and philosophy was far from complete, and we find that when faced with certain questions, he, too, introduced "intelligible" hypotheses, which revealed the "real nature" of the Universe. As an answer to the question why all planets revolve in the same sense and in approximately the same plane, he ascribed these regularities to a divine plan that manifests itself in the structure of the planetary system.

20. The Gradual Loosening of the Tie Between Philosophy and Science

A closer examination of Newton's thought reveals that the emerging split between philosophic and scientific discourse was well in the making. Newton derived the shape of planetary orbits from his law of gravitation through mathematics and did not care whether the law was "intelligible" in itself. But though he rejected any "philosophical" explanation of gravity, he did not refrain from attributing the mutual location of planetary orbits to a plan shaped by an "engineer of the Universe." This idea belongs unquestionably to "philosophy proper."

The development of modern science is characterized by a growing concern with practical applications. Physics developed out of the practical needs of the mechanical and electrical engineer; chemistry supplied principles behind industrial processes like the production of ammonia from nitrogen in the air; biology aided animal husbandry and human medicine. The important thing about the principles developed in these areas of science was that they worked, that they fulfilled the purposes for which they were intended. If they did, then the "scientist in the modern sense" did not much care whether or not they were "intelligible."

As less and less attention was paid to the requirement of intelligibility, it became more difficult to prove or even to understand the truth of scientific principles through mere reasoning and everyday experience. Those principles now tended away from descriptions of the "true" nature of the universe, and as a result were much less suitable as models of "goodness" or the "good society." The connections with ethics and religion were loosening. This did not mean that science could no longer provide directives for human conduct, but simply that the source of those directives was shifted from the science of the physical universe to the science of man: to biology, anthropology, and sociology.

21. IMMANUEL KANT AND THE GAP BETWEEN SCIENCE AND PHILOSOPHY

Philosophy proper became more and more independent of science proper, and pursued a progress of its own. Its task now was to present a picture of the essential features of the universe, and agreement of philosophical principles with observable facts became a secondary matter, though it was not utterly discarded. The emphasis, however, was now put on the suitability of the world picture as an example for human life; somehow the idea of truth and the idea of goodness had to be correlated. For example, educated people of the nineteenth century habitually discussed science in terms of the "iron law" of physical causality, but human actions were considered somehow exempt from this law; human beings had "freedom of choice." It was hardly conceivable that men could behave morally without this "freedom."

Around 1800, the width and depth of the gap between science and philosophy was emphatically affirmed by the chief representative of German Idealism, Immanuel Kant, one of the outstanding philosophers of all times. Kant was well acquainted with science and possessed extraordinary ability in the analysis of thought; at the same time, he had a sense of high moral responsibility.

Kant stated bluntly that the observable facts of the physical world are completely and satisfactorily described and systematized by "science proper"; "philosophy proper" can never tell us anything about them. It is the function of philosophy to erect a superstructure upon science, not to make statements about physical facts, but rather to ascribe "value" to certain kinds of human action. It was Kant's view that this moral interpretation of the universe is "true" in much the same sense as the statements of science are "true." The inquiry which seeks true statements beyond the domain of "science proper" and tries to obtain these statements from a "philosophical interpretation of science" is called a "metaphysical inquiry." The result of this kind of inquiry is a branch of systematic thought known as "metaphysics," whose function it is to provide us with rules for human conduct, to show us "the good."

But these rules are in no sense derived from an empirical study of the habits of men in a given environment, for that would be "science proper." Kant and his followers held that moral advice can be procured directly from the philosophical interpretation of the universe, without benefit of sociology or anthropology.

22. METAPHYSICS IS PRACTICAL RATHER THAN SPECULATIVE

A few passages from Kant's principal book, the famous *Critique of Pure Reason*, will illustrate how strong his feeling was on the sharp separation between science and philosophy:

> Reason is impelled by a tendency of its nature to go beyond the field of experience and to venture . . . by means of mere ideas to the uttermost limits of all knowledge. Moreover, it finds no rest until it has fulfilled its course and established a systematic whole of knowledge which exists by itself.

Kant was convinced, however, that metaphysics cannot furnish man with knowledge about the world of observable phenomena.

> Whatever discoveries may be made, he continues, they can never be applied to concrete facts, this means, in scientific research.

If this is so, the discoveries of "philosophy proper" are not attained by "speculative" reason. In other words, their purpose is not to give us theoretical knowledge about the world of observable facts; it is rather to serve what Kant called the "practical interests of reason." Let us see what he means by examining his principal examples.

"The highest aims of speculation," he says, "are the freedom of will, the immortality of the soul and the existence of God." But, Kant asks, what interests of reason are served by knowing that the will is free? If we employ scientific methods (speculative reason) to examine human action, we can only discover that it follows causal laws, just as other observable phenomena do. "In other words," writes Kant, "there is no helpful application (of the statement that the will is free) to the world of our experience." Statements like that about free will are therefore, in Kant's terms, "transcendent to speculative reason." Kant proceeds to ask why the proposition that our will is free is so strongly recommended to us by our reason despite its apparent lack of validity in the observable world. He answers that "its importance is probably connected with the practical." But what is ''practical'' about this statement, or about another one dealing with the ''spiritual'' nature of the soul? Just this, that if human actions were determined by ''iron laws'' of causality, then ''moral law'' could not possibly have any influence on behavior; it would be useless to direct men to act so as to achieve the ''supreme good.'' Kant was able to resolve the problem created by the ideas of determinism and freedom by postulating two worlds, that of phenomena or experience, and another which lies behind it, the world of ''things in themselves.'' Man as an observable phenomenon is subject to physical causality. As a ''thing in itself'' he is ''free'' and can follow the ''moral law.'' Since according to Kant, moral laws would have no meaning if our will were not free, it is ''practical'' to state that it *is* free; one should take the assertion of a ''free will'' away from the domain of theoretical or speculative reasoning where it must agree with the facts, and place it in the domain of practical reason, where confirmation by observation is not possible.

Kant's two worlds exemplify a complete split between science and philosophy.

BRIEF BIBLIOGRAPHY FOR THE SOCIOLOGY OF SCIENCE

BERNARD BARBER, *Smith College*

ROBERT K. MERTON, *Columbia University*

In broadest outline, the subject-matter of the sociology of science is *the dynamic interdependence* between science, as an ongoing social activity giving rise to empirically confirmed rational knowledge, and the environing social structure. The *reciprocal* relations between science and society are the object of inquiry, as those who have seriously applied themselves to studies in the sociology of science have been forced to recognize. But until very recently, the reciprocity of these relations has received uneven attention, the impact of science upon society eliciting much notice, and the impact of society upon science, little.

Possibly because it is so readily apparent, the impact of science upon the social structure, particularly through its technological by-products, has long been the object of concern if not of systematic study. It is plain to see that science is a dynamic force of social change, though not always of changes foreseen and desired. From time to time, during the last century or so, even physical scientists have emerged from their laboratories to acknowledge, with pride and wonder, or to disown, with horror and shame, the social consequences of their work. The explosion over Hiroshima only verified what everyone knew. Science has social consequences.

But if the consequences of science for society have been long perceived, the consequences of diverse social structures for science have not. Very few physical scientists and not many more social scientists have paid attention to the diverse influences of social structure upon the rate of development, the foci of interest and, perhaps, upon the very content of science. It is difficult to say why there is this reluctance to explore the bearing of its social environment upon science. The reluctance may come from the mistaken belief that to admit the sociological fact would be to jeopardize the autonomy of science. Perhaps it is believed that objectivity, so central a value in the ethos of science, is threatened by the fact that science is an organized social activity, that it presupposes support by society, that the measure of this support and the types of research for which it is given differ in different social structures, as does the recruitment of scientific talents. There may be something here of the sentiment that science remains the more pure and unsullied if it is implicitly conceived as developing in a social vacuum. Just as the word ''politics'' now carries for many the connotation of base corruption, so the phrase ''social contexts of science'' may connote for some physical scientists the intrusion of concerns alien to science proper.

Or perhaps the reluctance comes from the equally mistaken belief that to recognize these connections of science and society is to impugn the disinterested motives of the scientist. Their recognition may seem to imply that the scientist seeks, first and foremost, not the advancement of knowledge but the aggrandizement of self. This is a familiar type of error: the error lies in mistaking the level of motivational analysis for the level of institutional analysis. Scientists may be most variously motivated — by a disinterested desire to learn, by hope of economic gain, by active (or, as Veblen calls it, by idle) curiosity, by aggression or competition, by egotism or altruism. But the same motives in different institutional settings take different social expressions, just as different motivations in a given institutional setting may take approximately the same social expression. In one institutional context, egoism may lead a scientist to advance a branch of science useful for the military arts; in another institutional context, egoism may lead him to work on researches with apparently no military use. To consider how and how far social structures canalize the direction of scientific research is not to arraign the scientist for his motives.

But events of history have succeeded where the studies and writings of social scientists have failed. The course of recent history has made it increasingly difficult, even for scientists secluded in their laboratories and rarely moving about in the larger civil and political society, any longer to neglect the fact that science itself is variously dependent on the social structure. To select but a few of these events, there was first the emergence of Nazi Germany with its dramatic impact upon the nature, quality, and direction of the science cultivated in that country. Rather than recognizing this as an extreme and therefore illuminating case of a more general relationship, rather than seeing this as testimony to the fact that science requires particular forms of social structure in order to follow out its own genius, some physical scientists put this down as an exceptional and pathological case, with no implications for the more general situation. During the war, however, the marshalling of the forces of science led more scientists to recognize the *interplay* between their science and social structure. And most recently, the politicalizing of science in Soviet Russia has again led others to the same belated conclusion.

With these developments coming so hard on each other's heels as to seem almost one continuous event, many have come to recognize the connections between science and social structure among those who previously thought of these connections, if they thought of them at all, as a figment of Marxist sociology. (In his excellent little book, *On Understanding Science,* for example, James B. Conant refers to "the interconnection between science and society" as a subject "about which so much has been said in recent years by our Marxist friends.") Now, Marx and Engels did indeed set forth the general conception of these interconnections, and deplored the practice of writing "the history of the sciences as if they had fallen from the skies." But since the time of Marx and Engels, there has been distressingly little empirical study of the relations between science and social structure. The same old historical illustrations, grown venerable with age and threadbare with use, have been periodically trotted out to indicate that technological need sometimes leads scientists to focus upon distinctive problems of research. Through such overconformity to the early conceptions of Marx and Engels, piety has been expressed and the *advancement* of the sociology of science has been limited. A pattern of thought and writing has developed which would be appropriate, perhaps, for a religious group where changeless tradition is the thing and ancient revelation must remain intact. But this is scarcely a pattern appropriate to science, including social science, where the founding fathers are honored, not by zealous repetition of their early findings, but by extensions, modifications and, often enough, by rejections of some of their ideas and findings. In the sociology of science, as in other fields, we can profitably return to the wisdom of Whitehead's apothegm: "A science which hesitates to forget its founders is lost."

There is ample institutional evidence of this failure to follow up through empirical research the numerous and now widely recognized problems of the relations between science and the social structure; nowhere among the universities of this country is there an Institute for Research on the Social Relations of Science.

It is to these reciprocal relations between science and its social environment that the following bibliography is devoted.

Since this bibliography is designed to exemplify rather than to exhaust the materials of the sociology of science, it is all the more necessary to delimit its content closely. For one thing, the large literature on scientific method has been omitted as being peripheral rather than central to the subject. Second, and for the same reason, no effort is made to include a working bibliography of the history, as distinct from the sociology, of science, though it is often difficult to draw a useful rather than an arbitrary dividing line between the two. At times, historical accounts are interlarded with sociological analyses of the place of science in a society, and these have on occasion been included. Third, no effort has been made to itemize the large number of works in the "sociology of knowledge" dealing with ideologies, moral beliefs, philosophies, religious conceptions and the like.[1] Fourth, we have omitted biographies of scientists, though these are often instructive as raw materials for the sociologist of science.

The underlying principle of organization has been that of the main spheres of problems in the, as yet, slightly developed sociology of science. A more elaborate document would go on to organize these major categories in turn, according to the place, the time, and the particular sciences under examination. But this further categorization would clearly be inappropriate for the present brief bibliography. It should be noted also that the "sciences" to which reference is here made include mathematics and medicine, as well as the natural and physical sciences more strictly construed. On occasion, pertinent studies of the social aspects of technology are also included. And finally, though to a limited degree, studies of the interplay between society and social science also find a place.

Many of the references do not fall neatly and exclusively into one or another of the categories in the classification adopted here. In such instances, an effort is made to classify each item in the category to which it is primarily devoted.

---

[1] Brief bibliographies of the sociology of knowledge will be found in Karl Mannheim, *Ideology and Utopia,* and in Harry E. Barnes, Howard Becker and Frances Becker (eds.), *Contemporary Social Theories* (New York, 1940).

I. GENERAL THEORY IN THE SOCIOLOGY OF SCIENCE

Bernal, J. D., *The Social Functions of Science,* New York: The Macmillan Company, 1939.

Butterfield, Herbert, *The Origins of Modern Science, 1300-1800,* New York: The Macmillan Company, 1950.

Conant, James B., *On Understanding Science. An Historical Approach,* New Haven: Yale University Press, 1947.

————, *Science and Common Sense,* New Haven: Yale University Press, 1951.

Crowther, James Gerald, *The Social Relations of Science,* New York: The Macmillan Company, 1941.

Farrington, Benjamin, *Greek Science. Its Meaning for Us. I. Thales to Aristotle,* London: Pelican Books, 1944.

————, *Greek Science. Its Meaning for Us. II. Theophrastus to Galen,* London: Pelican Books, 1949.

Forbes, R. J., *Man the Maker. A History of Technology and Engineering,* New York: Henry Schuman, 1950.

Gittler, Joseph B., "Possibilities of a sociology of science," *Social Forces,* 18 (1940), 350-59.

Hartung, Frank E., "Sociological foundations of modern science," *Philosophy of Science,* 14 (1947), 68-95.

Hogben, Lancelot, *Mathematics for the Million,* New York: W. W. Norton & Company, Inc., 1940, rev. ed.

————, *Science for the Citizen; a Self-Educator Based on the Social Background of Scientific Discovery,* London: G. Allen & Unwin, Ltd., 1938.

Lilley, S., *Men, Machines, and History,* London: Cobbett Press, 1948.

————, "Social aspects of the history of science," *Archives Internationales d'Histoire des Sciences,* 28 (1948-49), 376-443.

Lovell, Bernard, *Science and Civilization,* London: Thomas Nelson and Sons, Ltd., 1939.

Malinowski, Bronislaw, *Magic, Science, and Religion,* Glencoe, Illinois: The Free Press, 1948.

Mason, O. T., *The Origins of Invention: A Study of Industry Among Primitive Peoples,* New York: Charles Scribner's Sons, 1915.

Merton, R. K., *Social Theory and Social Structure,* Glencoe, Illinois: The Free Press, 1949, Ch. VIII, "The Sociology of Knowledge"; Ch. XI, "Science and the Social Order."

Sarton, George, *The Life of Science. Essays in the History of Civilization,* New York: Henry Schuman, Inc., 1948.

Shryock, Richard H., "American indifference to basic science during the nineteenth century," *Archives Internationales d'Histoire des Sciences,* 28 (1948-49), 50-65.

Shryock, Richard H., *American Medical Research,* New York: Commonwealth Fund, 1947.

————, *The Development of Modern Medicine,* Philadelphia: The University of Pennsylvania Press, 1936.

————, "Trends in American medical research during the nineteenth century," *Proc. of the American Philosophical Society,* 19 (1947), 58-63.

Sigerist, Henry E., *Man and Medicine,* New York: W. W. Norton & Company, 1932.

Stauffer, R. C., ed., *Science and Civilization,* Madison: University of Wisconsin Press, 1949.

Stern, Bernhard J., *Society and Medical Progress,* Princeton: Princeton University Press, 1941.

Struik, Dirk J., *Yankee Science in the Making,* Boston: Little, Brown and Company, 1948.

Veblen, Thorstein, "The Place of Science in Modern Civilization" in *The Place of Science in Modern Civilization and Other Essays,* New York: The Viking Press, 1919.

Whitehead, A. N., *Science and the Modern World,* New York: The Macmillan Company, 1925.

Zilsel, Edgar, "The sociological roots of science," *American Journal of Sociology,* xlvii (1942), 544-562.

II. THE CULTURAL STRUCTURE OF SCIENCE

Bridgman, P. W., *Reflections of a Physicist,* New York: Philosophical Library, 1950.

Hardy, G. H., *A Mathematician's Apology,* Cambridge: Cambridge University Press, 1941.

Lundberg, George, *Can Science Save Us?,* New York: Longmans, Green and Company, 1947.

Lynd, Robert S., *Knowledge for What?,* Princeton: Princeton University Press, 1939.

Merton, R. K., *Social Theory and Social Structure,* Glencoe, Illinois: The Free Press, 1949. Ch. XII, "Science and Democratic Social Structure."

Otto, Max C., *Science and the Moral Life,* New York: Mentor Books, 1949.

Palmer, Archie M., *Survey of University Patent Policies. Preliminary Report,* Washington, D. C.: National Research Council, 1948.

Parsons, Talcott, *Essays in Sociological Theory,* Glencoe, Illinois: The Free Press, 1949. Ch. VIII, "The Professions and Social Structure."

Polanyi, M., *The Contempt of Freedom. The Russian Experiment and After,* London: Watts & Company, 1940.

————, *Science, Faith and Society,* New York: Oxford University Press, 1948.

Sarton, G., *The History of Science and The New Humanism,* Cambridge: Harvard University Press, 1937.

Simpson, George, "The scientist — technician or moralist?", *Philosophy of Science,* 17 (1950), 95-108.

Standen, Anthony, *Science is a Sacred Cow,* New York: E. P. Dutton and Company, 1950.

Waddington, C. H., *Science and Ethics,* London: George Allen and Unwin, 1942.

Weber, Max, "Science as a Vocation" in *From Max Weber: Essays in Sociology,* ed. and tr. by H. H. Gerth and C. W. Mills, New York: Oxford University Press, 1946.

Wigner, E. P., ed., *Physical Science and Human Values. A Symposium at the Princeton Bicentennial Conference,* Princeton: Princeton University Press, 1947.

III. THE SOCIAL ORGANIZATION OF SCIENCE

A. Recruitment and Social Composition of Scientists

————, "The Scientists," *Fortune,* October, 1948, 106-112, 166-176.

North, C. C., and Hatt, Paul K., "Jobs and Occupations: a Popular Evaluation" in Wilson, L. and Kolb, W. L., *Sociological Analysis,* New York: Harcourt Brace, 1949.

Reed, Elizabeth Wagner, "Productivity and attitudes of seventy scientific women," *American Scientist,* 38 (1950), 132-135.

Sibley, Elbridge, *The Recruitment of Social Scientists,* New York: Social Science Research Council, 1950.

Trytten, Merriam H., "The Mobilization of Scientists" in White, Leonard D., ed., *Civil Service in Wartime,* Chicago: The University of Chicago Press, 1945.

U. S. Department of Labor, Women's Bureau, *The Outlook for Women in Science,* Washington, D. C.: U. S. Government Printing Office, 1949.

U. S. President's Scientific Research Board, *A Program for the Nation.* Vol. One of *Science and Public Policy,* Washington, D. C.: U. S. Government Printing Office, August 27, 1947.

U. S. President's Scientific Research Board, *Manpower for Research,* Vol. Four of *Science and Public Policy,* Washington, D. C.: U. S. Government Printing Office, October 11, 1947.

Visher, Stephen Sargent, *Scientists Starred, 1903-1943, in American Men of Science*, Baltimore: The Johns Hopkins Press, 1947.

Znaniecki, Florian, *The Social Role of the Man of Knowledge*, New York: Columbia University Press, 1940.

B. PROFESSIONAL AND AMATEUR ORGANIZATION OF SCIENTISTS

Bates, Ralph S., *Scientific Societies in the United States*, New York: John Wiley & Sons, 1945.

Brown, Harcourt, *Scientific Organizations in Seventeenth Century France (1620-1680)*, Baltimore: The Williams & Wilkins Company, 1934.

Carr-Saunders, A. M. and Wilson, P. W., *The Professions*, Oxford: Clarendon Press, 1933.

Grobstein, Clifford, "The Federation of American Scientists," *Bulletin of the Atomic Scientists*, VI (1950), 58, 61.

Moulton, F. R., "The AAAS and Organized American Science," *Science*, 108 (1948), 573-577.

Ornstein, Martha, *The Role of the Scientific Societies in the Seventeenth Century*, Chicago: University of Chicago Press, 1938.

Stimson, Dorothy, *Scientists and Amateurs: A History of the Royal Society*, New York: Henry Schuman, Inc., 1948.

Thomas, W. Stephen, *The Amateur Scientist. Science as a Hobby*, New York: W. W. Norton & Company, 1942.

C. THE ORGANIZATION OF SCIENTIFIC WORK

1. *General Problems*

Bush, George P. and Hattery, Lowell H., eds., *Scientific Research: Its Administration and Organization*, Washington, D. C.: The American University Press, 1950.

Bush, Vannevar, *Science, The Endless Frontier*, Washington, D. C.: U. S. Government Printing Office, 1945.

Kowarski, L., "Psychology and structure of large-scale physical research," *Bulletin of the Atomic Scientists*, 5 (1949), Nos. 6-7.

McDonald, Ellice, *Research And Its Organization*, Newark, Delaware: Biochemical Research Foundation, n.d. (c. 1950).

2. *The University*

Cohen, I. Bernard, *Some Early Tools of American Science*, Cambridge: Harvard University Press, 1949.

Hornberger, Theodore, *Scientific Thought in the American College, 1638-1800*, Austin: The University of Texas Press, 1945.

Veblen, Thorstein, *The Higher Learning in America,* New York: Huebsch, 1918.

Wilson, Logan, *The Academic Man,* New York: Oxford University Press, 1942.

3. *Industry*

Bichowsky, F. Russell, *Industrial Research*, Brooklyn, New York: Chemical Publishing Company, 1942.

Boyd, T. A., *Research, the Pathfinder of Science and Industry,* New York: D. Appleton-Century Company, 1935.

Brown, Esther Lucile, *The Professional Engineer,* New York: Russell Sage Foundation, 1936.

Furnas, C. C., ed., *Research in Industry: Its Organization and Management,* New York: D. Van Nostrand, 1947.

Hawkins, Laurence A., *Adventure into the Unknown. The First Fifty Years of the General Electric Research Laboratory,* New York: William Morrow & Company, 1950.

Heath, H. Frank and Hetherington, A. L., *Industrial Research and Development in the United Kingdom; A Survey,* London: Faber & Faber, 1946.

Hertz, David B., *The Theory and Practice of Industrial Research,* New York: McGraw-Hill Book Company, 1950.

Hoover, Theodore J. and Fish, John C. L., *The Engineering Profession,* Palo Alto: Stanford University Press, 1947.

Killeffer, D. H., *The Genius of Industrial Research,* New York: Reinhold, 1948.

Mees, C. E. K. and Leermakers, J. A., *The Organization of Industrial Scientific Research,* 2nd edition, New York: McGraw-Hill Company, 1950.

Mills, John, *The Engineer in Society,* New York: D. Van Nostrand Company, Inc., 1946.

Standard Oil Company of California, *The Coordination of Motive, Men and Money in Industrial Research. A Survey of organization and business practices conducted by the Department on Organization of the Standard Oil Company of California:* privately printed, 1946.

U. S. National Resources Planning Board, *Research — A National Resource. II. Industrial Research,* Washington, D. C.: U. S. Government Printing Office, 1940.

U. S. National Resources Planning Board, *Research — A National Resource. III. Business Research,* Washington, D. C.: U. S. Government Printing Office, 1941.

4. *The Government*

Baxter, James Phinney, *Scientists Against Time,* Boston: Little, Brown and Company, 1946.

Gellhorn, Walter, *Security, Loyalty, and Science*, Ithaca, New York: Cornell University Press, 1950.

Harding, T. Swann, *Two Blades of Grass. A History of Scientific Development in the U. S. Department of Agriculture*, Norman: University of Oklahoma Press, 1947.

Hausrath, A. H., "Programs for fuller utilization of present resources of scientific personnel," *Science*, 107 (1948), 360-63.

Merton, Robert K., "Role of the intellectual in public bureaucracy," *Social Forces*, 23 (1945), 405-415.

Ridenour, Louis N., "Military support of American science, a danger?", *Bulletin of the Atomic Scientists*, August, 1947.

The Russell Sage Foundation, *Effective Use of Social Science Research in the Federal Services*, New York: The Russell Sage Foundation, 1950.

Smyth, Henry D., *Atomic Energy for Military Purposes. The Official Report of the Development of the Atom Bomb under the Auspices of the U. S. Government, 1940-45*, Princeton: Princeton University Press, 1945.

————, "The role of the national laboratories in atomic energy development," *Bulletin of the Atomic Scientists*, VI (1950), 5-8.

Stewart, Irvin, *Organizing Scientific Research for War. The Administrative History of the Office of Scientific Research and Development*, Boston: Little, Brown & Company, 1948.

U. S. Department of the Army, *Scientists in Uniform — World War II*, Washington, D. C.: U. S. Government Printing Office, 1948.

U. S. Department of Justice, *Investigation of Government Patent Practices and Policies* (3 vols.), Washington, D. C.: U. S. Government Printing Office, 1947.

U. S. National Resources Committee, *Research — A National Resource. I. Relation of the Federal Government to Research*, Washington, D. C.: U. S. Government Printing Office, 1938.

U. S. President's Scientific Research Board, *The Federal Research Program*, Vol. Two of *Science and Public Policy*, Washington, D. C.: U. S. Government Printing Office, September 27, 1947.

————, *Administration for Research*, Vol. Three of *Science and Public Policy*, Washington, D. C.: U. S. Government Printing Office, October 4, 1947.

————, *The Nation's Medical Research*, Vol. Five of *Science and Public Policy*, Washington, D. C.: U. S. Government Printing Office, October 18, 1947.

5. *The Foundations*

Hollis, E. V., *Philanthropic Foundations and Higher Education*, New York: Columbia University Press, 1938.

Keppel, F. P., *The Foundation; Its Place in American Life*, New York: The Macmillan Company, 1930.

Lindeman, E. C., *Wealth and Culture; A Study of One Hundred Foundations*, New York: Harcourt Brace, 1936.

IV. THE RELATIONS OF SCIENCE AND OTHER SOCIAL INSTITUTIONS

A. Religion

Agar, William M., *Catholicism and the Progress of Science*, New York: The Macmillan Company, 1940.

Cooper, J. M., "Catholics and scientific research," *Commonweal*, 42 (1945), 147-49.

Hsu, Francis L. K., *Magic and Science in Western Yunnan*, New York: Institute of Pacific Relations, 1943 (mimeographed).

Merton, R. K., *Science, Technology and Society in Seventeenth Century England*, OSIRIS, IV, Part 2, 1938, Bruges (Belgium), 1938.

Pelseneer, Jean, "L'origine Protestante de la science moderne," *Lychnos*, 1946-47, 246-48.

B. Economic Institutions

Clark, G. N., *Science and Social Welfare in the Age of Newton* (revised), Oxford: The Clarendon Press, 1949.

Hessen, B., "The Social and Economic Roots of Newton's 'Principia'," in *Science at the Cross Roads*, London: Kniga (England) Ltd., 1931.

Merton, R. K., "Science, population and society," *Scientific Monthly*, 44 (1937), 165-171.

Salant, W., "Science and society in ancient Rome," *Scientific Monthly*, 47 (1938), 525-35.

C. Political Institutions

1. *General Problems*

Baker, John R., *Science and the Planned State*, New York: The Macmillan Company, 1945.

Conant, James B., "Science and politics in the twentieth century," *Foreign Affairs*, 28 (1950), 189-202.

Farrington, Benjamin, "Prometheus bound: government and science in classical antiquity," *Science and Society*, 2 (1937), 435-447.

———, "The Great Science Debate," *Fortune*, 33 (June, 1946), 116-123, 236ff.

2. *Nazi Germany*

Goudsmit, Samuel A., *Alsos*, New York: Henry Schuman, Inc., 1947.

Gregory, Sir Richard, *Science in Chains*, London: Macmillan, 1941.

Hartshorne, E. Y., *German Universities and National Socialism*, Cambridge: Harvard University Press, 1937.

Needham, Joseph, *The Nazi Attack on International Science*, London: Watts & Company, 1941.

Simon, Leslie E., *German Research in World War II. An Analysis of the Conduct of Research*, New York: John Wiley & Sons, 1947.

3. *Soviet Russia*

Ashby, Eric, *Scientist in Russia*, New York: Penguin Books, 1947.

Bauer, Raymond, *The Conception of Man in Soviet Psychology*, Ph.D. Thesis, Harvard, 1950.

Crowther, J. G., *Soviet Science*, New York: E. P. Dutton, 1936.

Hughes, Francis, "Soviet invention awards," *Economic Journal* (London), 55 (1945), 291-97.

Huxley, Julian, *Heredity East and West: Lysenko and World Science*, New York: Henry Schuman, 1949.

Lefschetz, S., "Scientific Research in the U.S.S.R.: Mathematics," *The Annals of the American Academy of Political and Social Science*, 263 (1949), 139-40.

Leontief, W. W., Sr., "Scientific and technological research in Russia," *American Slavic and East European Review*, 4 (1945), 70-79.

Mazur, Nancy, *Science in Soviet Russia: A Sociological Analysis*, Undergraduate Honors Thesis, Smith College, 1951.

Mihailov, Alexander A., "The organization of scientific work in the U.S.S.R.," *American Review on the Soviet Union*, 8 (1947), 26-35.

Needham, Joseph and Davies, Jane Sykes, eds., *Science in Soviet Russia*, by seven British scientists, London: Watts & Company, 1942.

Oster, Gerald, "Scientific Research in the U.S.S.R.: Organization and Planning," *The Annals of the American Academy of Political and Social Science*, 263 (1949), 134-39.

Zirkle, Conway, ed., *Death of a Science in Russia. The Fate of Genetics as Described in Pravda and Elsewhere*, Philadelphia: University of Pennsylvania Press, 1949.

V. SOCIAL AND PSYCHOLOGICAL COMPONENTS OF SCIENTIFIC WORK: INVENTION AND DISCOVERY

Baker, John R., *The Scientific Life,* London: George Allen and Unwin, Ltd., 1942.

Bernard, Claude, *An Introduction to the Study of Experimental Medicine* (translated by Henry C. Greene), New York: Henry Schuman, 1927.

Bright, Arthur A., Jr., and Maclaurin, W. Rupert, "Economic factors influencing the development and introduction of the fluorescent lamp," *Journal of Political Economy,* 51 (1943), 429-450.

Cannon, Walter Bradford, *The Way of an Investigator. A Scientist's Experiences in Medical Research,* New York: W. W. Norton and Company, 1945.

Cohen, I. Bernard, *Science, Servant of Man,* Boston: Little, Brown and Company, 1948.

Epstein, Ralph C., "Industrial invention: heroic or systematic?", *Quarterly Journal of Economics,* XL (1925-26), 232-72.

Farber, Eduard, "Chemical discoveries by means of analogies," ISIS, 41 (1950), 20-26.

————, "Nylon," *Fortune,* 22 (July, 1940), 57-60, 114-16.

George, W. H., *The Scientist in Action,* London: Williams and Norgate, 1936.

Gildeon, S., *Mechanization Takes Command,* New York: Oxford University Press, 1948.

Gilfillan, S. C., *Inventing the Ship,* Chicago: Follett Publishing Company, 1935.

————, *The Sociology of Invention,* Chicago: Follett Publishing Company, 1935.

Hadamard, Jacques, *The Psychology of Invention in the Mathematical Field,* Princeton: Princeton University Press, 1945.

Hatfield, H. Stafford, *The Inventor and His World,* London: Pelican Books, 1948.

Kroeber, A. L., *Configurations of Culture Growth,* Berkeley: University of California Press, 1944.

Maclaurin, W. Rupert, *Invention and Innovation in the Radio Industry,* New York: The Macmillan Company, 1949.

Mach, E., "On the part played by accident in invention and discovery," *The Monist,* 6 (1896), 161-75.

Mumford, Lewis, *Technics and Civilization,* New York: Harcourt, Brace and Company, 1934.

Ogburn, W. F. and Thomas, D., "Are inventions inevitable?", *Political Science Quarterly,* 37 (1922), 83-98.

Platt, W. and Baker, R. A., "The relation of the scientific 'hunch' to research," *Journal of Chemical Education,* VIII (1931), 1969-2002.

Rossman, Joseph, *The Psychology of the Inventory. A Study of the Patentee,* Washington, D. C.: Inventors Publishing Company, revised edition, 1931.

Spengler, Oswald, *Man and Technics,* New York: Alfred A. Knopf, 1932.

Stamp, Sir Josiah, *Some Economic Factors in Modern Life,* London: P. S. King and Son, 1929.

Taussig, F. W., *Inventors and Money-Makers,* New York: The Macmillan Company, 1915.

Watson, David Lindsay, *Scientists Are Human,* London: Watts and Company, 1938.

White, Lynn, Jr., "Technology and invention in the Middle Ages," *Speculum: a Journal of Mediaeval Studies,* 15 (1940), 141-59.

VI. THE SOCIAL CONSEQUENCES OF SCIENCE

Ayres, Clarence Edwin, *Science, The False Messiah,* New York: The Bobbs-Merrill Company, 1927.

Brozen, Yale, *Social Implications of Technological Change: A Research Planning Report.* Mimeographed: available from the Social Science Research Council, New York.

Haldane, J. B. S., *Callinicus: A Defence of Chemical Warfare,* London: Kegan Paul, 1924.

————, *Daedalus, or Science and the Future,* New York: E. P. Dutton and Company, 1924.

Harding, T. Swann, *The Degradation of Science,* New York: Farrar and Rinehart, 1931.

Huxley, Julian, *Science and Social Needs,* New York: Harper and Brothers, 1935.

Kottke, Frank Joseph, *Electrical Technology and the Public Interest. A Study of Our National Policy Toward the Development and Application of Inventions,* Washington, D. C.: American Council on Public Affairs, 1944.

Leighton, Alexander H., *Human Relations in a Changing World. Observations on the Use of the Social Sciences,* New York: E. P. Dutton and Company, 1949.

Lilley, S., "Can prediction become a science?", *Discovery,* November, 1946, 336-40.

Merton, R. K., "The machine, the worker, and the engineer," *Science,* 105 (1947), 79-84.

Newman, James R. and Miller, Byron S., *The Control of Atomic Energy. A Study of Its Social, Economic and Political Implications,* New York: McGraw-Hill Company, 1948.

Ogburn, William Fielding, *Social Change,* New York: B. W. Huebsch, 1922.

———, *The Social Effects of Aviation,* Boston: Houghton Mifflin Company, 1946.

———, ed., *Technology and International Relations,* Chicago: University of Chicago Press, 1949.

Russell, Bertrand, *The Impact of Science on Society,* New York: Columbia University Press, 1951.

Shryock, Richard H., "Freedom and Interference in Medicine," *The Annals of the American Academy of Political and Social Science,* 200 (November, 1938).

Smith, Elliott Dunlap, *Technology and Labor. A Study of the Human Problems of Labor Saving,* New Haven: Yale University Press, 1939.

Soddy, Sir Frederick, *The Frustration of Science.* A collection of essays by Sir Daniel Hall, J. G. Crowther, J. D. Bernal, V. H. Mottram, Enid Charles, P. A. Gorer, and P. M. S. Blackett, with a foreword by Sir Frederick Soddy, New York: W. W. Norton and Company, Inc., 1935.

Stamp, Sir Josiah, *The Science of Social Adjustment,* London: The Macmillan Company, 1937.

Stern, Bernard J., "Resistances to the Adoption of Technological Inventions" in *Technological Trends and National Policy,* Washington, D. C.: U. S. Government Printing Office, 1937.

———, "Restraints upon the utilization of inventions," *The Annals of the American Academy of Political and Social Science,* 200 (November, 1938).

———, *Social Factors in Medical Progress,* New York: Columbia University Press, 1927.

U. S. National Resources Committee, *Technological Trends and National Policy, Including the Social Implications of New Inventions,* Washington, D. C.: U. S. Government Printing Office, 1937.

Veblen, Thorstein, *The Engineers and the Price System,* New York: B. W. Huebsch, Inc., 1921.

Wiener, Norbert, *The Human Use of Human Beings: Cybernetics and Society,* Boston: Houghton Mifflin Company, 1950.

BRIEF BIBLIOGRAPHY OF FORMAL LOGIC

Alonzo Church

1. HISTORY OF LOGIC.

1.1. Carl Prantl, *Geschichte der Logik im Abendlande,* 4 vols., Leipzig 1855-70. The only comprehensive history of ancient and medieval logic, this is in need of drastic revision in the light of developments since the author's day.

1.2. Heinrich Scholz, *Geschichte der Logik,* Berlin 1931, vii + 78 pp.

1.3. Jørgen Jørgensen, *A Treatise of Formal Logic,* 3 vols., Copenhagen and London 1931.

1.4. I. M. Bocheński, *L'état et les besoins de l'histoire de la logique formelle,* Proceedings of the Tenth International Congress of Philosophy,[1] Amsterdam 1949, pp. 1062-1064. A Polish translation in Przegląd Filozoficzny, vol. 44 (1948), pp. 389-394, adds a useful bibliography of works on the history of logic.

1.5. I. M. Bocheński, *Ancient Formal Logic,* Amsterdam 1951, ix + 122 pp.

1.6. Ernst Kapp, *Greek Foundations of Traditional Logic,* New York 1942, viii + 95 pp.

1.7. W. D. Ross, *Aristotle,* London 1923. Chapter II, *Logic,* pp. 20-61.

1.8. Albrecht Becker, *Die Aristotelische Theorie der Möglichkeitsschlüsse* (dissertation Münster 1932), Berlin 1933, 98 pp.

1.8a. Jan Łukasiewicz, *Aristotle's Syllogistic from the Standpoint of Modern Formal Logic,* Oxford, London, and New York 1951, xi + 141 pp.

1.9. I. M. Bocheński, *La Logique de Théophraste,* Fribourg 1947, 138 pp.

1.10. Alexander Rüstow, *Der Lügner, Theorie, Geschichte und Auflösung* (dissertation Erlanger 1908), Leipzig 1910, iv + 145 pp.

1.11. Jan Łukasiewicz, *Zur Geschichte der Aussagenlogik,* Erkenntnis, vol. 5 (1935), pp. 111-131.

1.12. Benson Mates, *Diodorean implication,* The Philosophical Review, vol. 58 (1949), pp. 234-242.

1.13. James W. Stakelum, *Galen and the Logic of Propositions* (dissertation Angelicum), Rome 1940, xiv + 97 pp.

1.14. Benson Mates, *Stoic logic and the text of Sextus Empiricus,* American Journal of Philology, vol. 70 (1949), pp. 290-298.

1.15. René van den Driessche, *Sur le "De Syllogismo Hypothetico" de Boèce,* Methodos, vol. 1 (1949), pp. 293-307.

1.16. Karl Dürr, *The Propositional Logic of Boethius,* Amsterdam 1951, x + 79 pp.

1.17. Philotheus Boehner, *Elements and Systems of Scholastic Logic,* forthcoming book.

1.18. Ernest A. Moody, *The Logic of William of Ockham,* London 1935, xiv + 322 pp.

1.19. J. Salamucha, *Die Aussagenlogik bei Wilhelm Ockham,* translated by Johannes Bendiek, Franziskanische Studien, vol. 32 (1950), pp. 97-134.

1.20. C. I. Lewis, *A Survey of Symbolic Logic,* Berkeley, California, 1918, vi + 406 pp. The first chapter has a valuable account of the logic of Leibniz, Lambert, von Holland, Castillon, De Morgan, Boole, Jevons, Peirce, and others. At the end of the book are English translations of two important fragments from Leibniz, and a useful bibliography of contributions to mathematical logic up to 1917.

1.21. Louis Couturat, *La Logique de Leibniz d'après des Documents Inédits,* Paris 1901, xiv + 608 pp.

1.22. Karl Dürr, *Leibniz' Forschungen im Gebiet der Syllogistik,* Leibniz zu seinem 300. Geburtstag 1646-1946, Lieferung 5, Berlin 1949, 40 pp.

1.23. Karl Dürr, *Die Logistik Johann Heinrich Lamberts,* Festschrift Andreas Speiser, Zurich 1945, pp. 47-65.

1.24. P.E.B. Jourdain, *The development of theories of mathematical logic and the principles of mathematics,* The Quarterly Journal of Pure and Applied Mathematics, vols. 41, 43, and 44 (1910-13). Accounts of the work of Leibniz, Boole, Jevons, MacColl, Frege, Peano.

1.25. Heinrich Scholz, *Die Wissenschaftslehre Bolzanos,* Semester-Berichte (Münster i. W.), winter 1936-7, pp. 1-53.

1.26. Heinrich Scholz, *Was ist ein Kalkül und was hat Frege für eine pünktliche Beantwortung dieser Frage geleistet?,* ibid., summer 1935, pp. 16-47.

1.27. Alonzo Church, *Schröder's anticipation of the simple theory of types,* preprinted 1939 as from The Journal of Unified Science (Erkenntnis), vol. 9, 4 pp.

1.28. W. V. Quine, *Whitehead and the rise of modern logic,* The Philosophy of Alfred North Whitehead, edited by P. A. Schilpp, Evanston and Chicago 1941, pp. 127-163.

Also 3.3, 3.4, 3.7, 4.3 (as a useful source of information about the history of traditional logic, from the point of view of its day), 4.4 (on Aristotle), 5.6 (on Peirce), 6.8 (for the history of mathematical logic), 33.2 (on Frege).

The above-named publications are concerned with the history of logic in the West. To these we add the following on the history of Indian logic:

1.29. St. Schayer, *Über die Methode der Nyāya-Forschung,* Festschrift Moriz Winternitz, Leipzig 1933, pp. 247-257.

1.30. St. Schayer, *Studien zur indischen Logik,* Bulletin International de l'Académie Polonaise des Sciences et des Lettres, classe de philologie, classe d'histoire et de philosophie, année 1932, pp. 98-102, and année 1933, pp. 90-96.

1.31. D. H. H. Ingalls, *Materials for the Study of Navya-nyāya Logic,* Cambridge, Mass., 1951, 182 pp.

2. BIBLIOGRAPHY.

2.1. Alonzo Church, *A bibliography of symbolic logic,* The Journal of Symbolic Logic,[2] vols. 1 and 3 (1936-38). Comprehensive of publications related to mathematical logic (up to 1936). After that in vol. 3, shorter lists of additions and corrections were published in vol. 4, pp. 38, 136, 174-175; vol. 5, pp. 132, 170; vol. 7, pp. 103-104; vol. 8, pp. 59-60; vol. 9, p. 56; vol. 10, p. 135; vol. 12, p. 147; vol. 14, p. 208; vol. 16, pp. 234-235.

2.2. С. А. Яновская (S. A. Ánovskaá), *Foundations of mathematics and mathematical logic* (Russian), Математика в СССР за Тридцать Лет 1917-1947 (Mathematics in the USSR for the Thirty Years 1917-1947), Moscow and Leningrad 1948, pp. 9-50. A valuable source of information about Russian work in mathematical logic, much of which is not easily obtainable. See review by George L. Kline in JSL, vol. 16 (1951), pp. 46-48.

2.3. E. W. Beth, *Symbolische Logik und Grundlegung der Exakten Wissenschaften,* Bibliographische Einführungen in das Studium der Philosophie no. 3, Bern 1948, 28 pp.

Also 1.20, 18.1.

3. MODERN EDITIONS OF SCHOLASTIC WORKS.

3.1. Peter Abelard: *Petri Abaelardi Dialectica,* Ouvrages Inédits d'Abélard publiés par M. Victor Cousin, Paris 1836, pp. 171-503. Cf. 1.16, § 12.

3.2. Albertus Magnus: *B. Alberti Magni Opera Omnia* (ed. Borgnet), 38 vols., Paris 1890-99.

3.3. Petrus Hispanus: *Summulae Logicales,* edited by I. M. Bocheński, Turin 1947, xxiii + 143 pp.

3.4. Petrus Hispanus: *The Summulae Logicales of Peter of Spain,* by Joseph P. Mullally, Notre Dame 1945, civ + 172 pp. The *Introduction* contains a life of Petrus Hispanus, a survey of Tracts I-VI of *Summulae Logicales* (in the numbering of Prantl (1.1)), and an account of the antecedents and nature of Tract VII. There follows "a practical edition and a translation of Tract VII" (including a final section on *exponibilia,* omitted from 3.3 as spurious).

3.5. Thomas Aquinas: *Sancti Thomae Aquinatis Opera Omnia,* 25 vols., Parma 1852-73; reprinted New York 1948-50.

3.6. Joannes Duns Scotus: *Joannis Duns Scoti Opera Omnia,* 26 vols., Paris 1891-95.

3.7. William of Ockham: *The Tractatus de Praedestinatione et de Praescientia Dei et de Futuris Contingentibus of William Ockham,* edited with a study on the mediaeval problem of a three-valued logic by Philotheus Boehner, St. Bonaventure, N. Y., 1945, xi + 139 pp. As appendices are several passages from other works of Ockham, a passage from Peter Aureoli, and one from Gregory a Rimini.

4. POST-SCHOLASTIC TRADITIONAL LOGIC.

4.1. Antoine Arnauld and others, *La Logique ou l'Art de Penser,* 1st edn. Paris 1662; reprinted in Arnauld's *Oeuvres,* vol. 41 (1780), pp. 125-412; reprinted Paris 1878. *The Port-Royal Logic,* English translation by T. S. Baynes, 2nd edn. London 1851; 8th edn. Edinburgh and London c. 1885.

4.2. John Stuart Mill, *A System of Logic, Ratiocinative and Inductive,* London 1843; 4th edn. (2 vols.) London 1856; 8th edn. New York and London 1881.

4.3. Friedrich Ueberweg, *System der Logik und Geschichte der Logischen Lehren,* Bonn 1857; 5th edn. Bonn 1882. *System of Logic and History of Logical Doctrines,* English translation by T. M. Lindsay, London 1871, xx + 590 pp.

4.4. H. W. B. Joseph, *An Introduction to Logic,* 2nd edn. Oxford 1916, xii + 608 pp.

4.5. Bernard Bolzano, *Wissenschaftslehre,* 4 vols., Sulzbach 1837; reprinted Leipzig 1914-15 and 1929-31.

4.6. Franz Brentano, *Psychologie vom Empirischen Standpunkte,* Leipzig 1874; the second part, as *Von der Klassifikation der Psychischen Phänomene,* reprinted 1911 with added appendices; the complete work, with some further additions, reprinted in 2 vols., Leipzig 1925. *Psychologie du Point de Vue Empirique,* French translation by Maurice de Gandillac, Paris 1944.

4.7. Franz Hillebrand, *Die Neuen Theorien der Kategorischen Schlüsse,* Vienna 1891, vi + 102 pp.

4.8. J. N. Keynes, *Studies and Exercises in Formal Logic,* 1st edn. London 1884, xii + 414 pp.; 2nd edn. London and New York 1887; 3rd edn. London and New York 1894; 4th edn. London 1906, xxiv + 548 pp.

Of these, 4.1 and 4.2 are important classical works. The distinction of comprehension and extension was first introduced in 4.1, and the related distinction of connotation and denotation of names in 4.2. (Mill's treatment of induction is outside the scope of this bibliography.) 4.3 and 4.4 are selected as typical of the best of more recent works of traditional character. Bolzano's 4.5 and Brentano's 4.6 are departures from traditional logic, tending in some respects in the direction of mathematical logic (in spite of Brentano's expressed opposition to mathematical logic, which seems to have been based on misinformation). The ideas of Brentano about immediate inference and the syllogism are developed in more detail by Hillebrand in 4.7. Keynes's 4.8 is primarily a text-book of traditional formal logic, but at the same time a synthesis of traditional logic and one branch of mathematical logic (the elementary algebra of classes). Later text-books which (in various ways) combine a traditional treatment with some attention to mathematical logic are here omitted, in order to make room for works on mathematical logic proper.

5. PUBLICATIONS OF HISTORICAL INTEREST IN THE DEVELOPMENT OF MATHEMATICAL LOGIC.

5.1. G. W. v. Leibniz: *Opuscules et Fragments Inédits de Leibniz*, edited by Louis Couturat, Paris 1903.

5.2. Augustus De Morgan, *Formal Logic*, London 1847, xvi + 336 pp.; reprinted London 1926.

5.3. George Boole, *The Mathematical Analysis of Logic*, London and Cambridge 1847, 82 pp.; reprinted Oxford 1948.

5.4. George Boole, *An Investigation of the Laws of Thought*, London 1854, v + iv + 424 pp.; reprinted Chicago and London 1916; reprinted Chicago and London 1940; reprinted New York 1951.

5.5. W. S. Jevons, *Pure Logic*, London 1864, 87 pp.; reprinted in *Pure Logic and Other Minor Works*, London and New York 1890.

5.6. C. S. Peirce: *Collected Papers*, vols. 2-4, edited by Paul Weiss, Cambridge, Mass., 1932-33.

5.7. Ernst Schröder, *Der Operationskreis des Logikkalkuls*, Leipzig 1877, v + 37 pp.

5.8. Gottlob Frege, *Begriffsschrift*, Halle 1879, viii + 88 pp.

5.9. Gottlob Frege, *Die Grundlagen der Arithmetik*, Breslau 1884; reprinted Breslau 1934, xi + 119 pp. *The Foundations of Arithmetic*, reprint with English translation in parallel by J. L. Austin, Oxford and New York 1950.

5.10. Giuseppe Peano, *Arithmetices Principia*, Turin 1889, xvi + 20 pp.

5.11. Ernst Schröder, *Vorlesungen über die Algebra der Logik*, 3 vols., Leipzig 1890-1905.

5.12. Giuseppe Peano and others: articles on mathematical logic in *Rivista di Matematica*, vols. 1-8, Turin 1891-1906.

5.13. Gottlob Frege, *Grundgesetze der Arithmetik,* 2 vols., Jena 1893 and 1903. English translations of some passages from this, by P. E. B. Jourdain, The Monist, vol. 25 (1915), pp. 481-494, vol. 26 (1916), pp. 182-199, vol. 27 (1917), pp. 114-127; and by Max Black, The Philosophical Review, vol. 59 (1950), pp. 77-93, 202-220, 332-345. (These translations do not, however, include the important technical portions of the work.)

5.14. Giuseppe Peano, *Notations de Logique Mathématique, Introduction au Formulaire de Mathématique,* Turin 1894, 52 pp.

5.15. Giuseppe Peano and others, *Formulaire de Mathématiques,* published in a series of five volumes (as they are called—more correctly five editions), Turin 1895-1908.

5.16. Bertrand Russell, *The Principles of Mathematics,* Cambridge, England, 1903, xxix + 534 pp.; 2nd edn. London 1937 and New York 1938, reprinted London 1942, 1948, 1950.

5.17. Ernst Zermelo, *Untersuchungen über die Grundlagen der Mengenlehre,* Mathematische Annalen, vol. 65 (1908), pp. 261-281.

5.18. Bertrand Russell, *Mathematical logic as based on the theory of types,* American Journal of Mathematics,[3] vol. 30 (1908), pp. 222-262.

5.19. A. N. Whitehead and Bertrand Russell, *Principia Mathematica,* 3 vols., Cambridge, England, 1910-13; 2nd edn. Cambridge 1925-27, reprinted Cambridge 1950.

6. TEXT-BOOKS AND GENERAL TREATISES OF MATHEMATICAL LOGIC.

6.1. Alfred Tarski, *Einführung in die Mathematische Logik,* Vienna 1937, x + 166 pp. *Introduction to Logic,* revised and enlarged English edition (translation by Olaf Helmer), New York 1941, xviii + 239 pp.; 2nd English edn., with minor revisions, New York 1946. An excellent text-book at a very elementary level, addressed primarily to students of mathematics. The English edition is recommended.

6.2. Stanislas Zaremba, *La Logique des Mathématiques,* Paris 1926, 52 pp. Addressed to mathematicians, this is not a text-book or a comprehensive treatise, but an outline of a program for mathematical logic.

6.3. John C. Cooley, *A Primer of Formal Logic,* New York 1942, xi + 378 pp. Elementary text-book for students of philosophy.

6.4. W. V. Quine, *Methods of Logic,* New York 1950, xx + 264 pp. Recommended as a text-book for students of philosophy.

6.5. I. M. Bocheński, *Nove Lezioni di Logica Simbolica,* Rome 1938, 184 pp.

6.6. I. M. BOCHEŃSKI, *Précis de Logique Mathématique*, Bussum 1949, 90 pp. An excellent outline suffers from faults due to excessive condensation.

6.7. RUDOLF CARNAP, *Abriss der Logistik*, Vienna 1929, vi + 114 pp. Very condensed, intended to introduce the student rapidly to the technique and applications of mathematical logic.

6.8. ROBERT FEYS, *De Ontwikkeling van het Logisch Denken*, Antwerp and Nijmegen 1949, 220 pp. Excellent historical introduction.

6.9. ROBERT FEYS, *Logistiek, Geformaliseerde Logica*, vol. I, Antwerp and Nijmegen 1944, 340 pp. First volume of a proposed comprehensive manual.

6.10. W. V. QUINE, *O Sentido da Nova Lógica*, São Paulo 1944, 252 pp. An excellent text-book and expository work, philosophically oriented.

6.11. D. HILBERT and W. ACKERMANN, *Grundzüge der Theoretischen Logik*, 3rd edn., Berlin-Göttingen-Heidelberg 1949, viii + 155 pp. *Principles of Mathematical Logic*, unauthorized English translation of the second edition, New York 1950. Standard text-book for students of mathematics. The third German edition is recommended.

6.12. HEINRICH SCHOLZ, *Vorlesungen über Grundzüge der Mathematischen Logik*, 2 vols., Münster i. W. 1949; revised edition, Münster i. W. 1950-51. An intermediate mathematical text-book, like 6.11 especially valuable as an introduction to 6.18.

6.13. RUDOLF CARNAP, *Logische Syntax der Sprache*, Vienna 1934, xi + 274 pp. *The Logical Syntax of Language*, revised and enlarged English edition (translation by Amethe von Zeppelin), New York and London 1937. An account of the fundamentals of mathematical logic (less elementary than 6.7), and at the same time of the author's philosophical views. The latter have since undergone important modifications—cf. 32.4, 33.5, 32.6.

6.14. W. V. QUINE, *Mathematical Logic*, New York 1940, xiii + 348 pp.; revised second printing, Cambridge, Mass., 1947, xi + 340 pp.; revised edition, Cambridge, Mass., 1951, xii + 346 pp. A relatively advanced text-book, and a treatise on a system due to the author as an alternative to type theory or set theory. The edition of 1951 is recommended.

6.15. JACQUES HERBRAND, *Recherches sur la Théorie de la Démonstration* (dissertation Paris 1930), Warsaw 1930, 128 pp.

6.16. ANDRZEJ MOSTOWSKI, *Logika Matematyczna*, Warsaw and Breslau 1948, viii + 388 pp. Excellent advanced text-book for students of mathematics. An English translation is planned.

6.17. ALONZO CHURCH, *Introduction to Mathematical Logic*, part I, Princeton, N. J., 1944, vi + 118 pp. As planned, the complete work is a systematic treatise, and text-book for students with substantial mathematical background. A much enlarged and revised edition of part I is in process of publication.

6.18. D. Hilbert and P. Bernays, *Grundlagen der Mathematik,* 2 vols., Berlin 1934 and 1939. Comprehensive treatise of mathematical logic and Hilbertian proof theory.

7. PROPOSITIONAL CALCULUS.

7.1. E. L. Post, *Introduction to a general theory of elementary propositions,* AJM, vol. 43 (1921), pp. 163-185.

7.2. Paul Bernays, *Axiomatische Untersuchung des Aussagen-Kalkuls der "Principia Mathematica."* Mathematische Zeitschrift, vol. 25 (1926), pp. 305-320.

7.3. Jan Łukasiewicz, *Elementy Logiki Matematycznej,* Warsaw 1929, viii + 200 pp.

7.4. Jan Łukasiewicz and Alfred Tarski, *Untersuchungen über den Aussagenkalkül,* Comptes Rendus des Séances de la Société des Sciences et des Lettres de Varsovie, class III,[4] vol. 23 (1930), pp. 30-50.

7.5. László Kalmár, *Über die Axiomatisierbarkeit des Aussagenkalküls,* Acta Scientiarum Mathematicarum, vol. 7 (1934-5), pp. 222-243.

7.6. W. V. Quine, *Completeness of the propositional calculus,* JSL, vol. 3 (1938), pp. 37-40.

7.7. Jan Łukasiewicz, *Die Logik und das Grundlagenproblem,* Les Entretiens de Zurich,[5] edited by F. Gonseth, Zurich 1941, pp. 82-108.

7.8. Stanisław Jaśkowski, *Trois contributions au calcul des propositions bivalent,* Studia Societatis Scientiarum Torunensis, sectio A, vol. 1 (1948), pp. 1-15.

7.9. Leon Henkin, *Fragments of the propositional calculus,* JSL, vol. 14 (1949), pp. 42-48. See also Maurice L'Abbé, JSL, vol. 16, pp. 43-45.

Also 6.1 (2nd English edn.), 6.11, 6.15, 6.17, 6.18.

8. FUNCTIONAL CALCULUS OF FIRST ORDER.

8.1. Kurt Gödel, *Die Vollständigkeit der Axiome des logischen Funktionenkalküls,* Monatshefte für Mathematik und Physik,[6] vol. 37 (1930), pp. 349-360.

8.2. Leon Henkin, *The completeness of the first-order functional calculus,* JSL, vol. 14 (1949), pp. 159-166.

8.3. Gonzalo Zubieta R., *Sobre el Cálculo Funcional de Primer Orden* (dissertation Mexico 1950), vi + 35 pp.

8.4. GONZALO ZUBIETA R., *Sobre la substitución de las variables funcionales en el cálculo funcional de primer orden,* Boletin de la Sociedad Matemática Mexicana, vol. 7 (1950), pp. 1-21.

Also 6.11, 6.12, 6.15, 6.16, 6.17, 6.18, 26.2, 26.3, 26.5. See further 9 and 11.

9. DECISION PROBLEM OF THE FUNCTIONAL CALCULUS OF FIRST ORDER.

9.1. JACQUES HERBRAND, *Sur le problème fondamental de la logique mathématique,* CRV, vol. 24 (1931), pp. 12-56.

9.2. KURT GÖDEL, *Zum Entscheidungsproblem des logischen Funktionenkalküls,* MMP, vol. 40 (1933), pp. 433-443.

9.3. ALONZO CHURCH, *A note on the Entscheidungsproblem,* JSL, vol. 1 (1936), pp. 40-41, 101-102.

9.4. LÁSZLÓ KALMÁR, LÁSZLÓ KALMÁR and JÁNOS SURÁNYI, *On the reduction of the decision problem,* JSL, vol. 4 (1939), pp. 1-9, vol. 12 (1947), pp. 65-73, vol. 15 (1950), pp. 161-173.

9.5. JÁNOS SURÁNYI, *Zur Reduktion des Entscheidungsproblems des logischen Funktionenkalküls* (Hungarian with German abstract), Matematikai és Fizikai Lapok, vol. 50 (1943), pp. 51-74. See review by Rózsa Péter in JSL, vol. 9 (1944), pp. 22-24.

9.6. JÁNOS SURÁNYI, *Reduction of the decision problem to formulas containing a bounded number of quantifiers only,* XCP, pp. 759-762.

9.7. LÁSZLÓ KALMÁR, *Contributions to the reduction theory of the decision problem, First paper,* Acta Mathematica Academiae Scientiarum Hungaricae, vol. 1 no. 1 (1950), pp. 64-73.

9.8. ALONZO CHURCH, *Special cases of the decision problem,* Revue Philosophique de Louvain, vol. 49 (1951), pp. 203-221. An expository summary of solutions in special cases. Errata: in each of the two lines of the displayed formula on page 206, insert three dots after the last bold dot in the line; and six lines below the formula, for μ read ν. An error regarding Case 7 (p. 210) is corrected by Ackermann, JSL, vol. 17, pp. 73-74.

Also 6.11, 6.15, 6.17, 6.18, 14.3. See further 10.

10. DECISION PROBLEMS IN GENERAL.

10.1. ALFRED TARSKI, *On essential undecidability* (abstract), JSL, vol. 14 (1949), pp. 75-76.

10.2. W. V. QUINE, *On decidability and completeness,* Synthese, vol. 7 (1948-9), pp. 441-446.

10.3. ANTONI JANICZAK, *A remark concerning decidability of complete theories,* JSL, vol. 15 (1950), pp. 277-279.

Results concerning various particular decision problems (other than that of the functional calculus of first order) are here omitted for lack of space.

11. ABSOLUTE LOGIC.

Papers in Russian by D. A. Bočvar and P. S. Novikov. See reviews in JSL, vol. 11, p. 129 (second review); vol. 12, p. 27; vol. 13, p. 170. The "absolute logic" of Bočvar offers an interesting fresh approach to the problem of the logical paradoxes, though his results so far obtained in connection with it are of preliminary character and not of major importance. The principal result claimed (without proof) by Novikov is in error, at least as reported in JSL—being subject to the counter-example $(Ep_2)\ (x_2, x)\ [p_2\ (x_2, x) \equiv \sim x_2\ (x, x)]$, as pointed out by A. A. Barnes, Jr.

12. THEORY OF TYPES.

12.1. Alfred Tarski, *Einige Betrachtungen über die Begriffe der ω-Widerspruchsfreiheit und der ω-Vollständigkeit,* MMP, vol. 40 (1933), pp. 97-112.

12.2. Gerhard Gentzen, *Die Widerspruchsfreiheit der Stufenlogik,* Mathematische Zeitschrift, vol. 41 (1936), pp. 357-366. Detailed statement of a consistency proof for type theory without axiom of infinity (previously given more briefly by Herbrand 6.15, Tarski 12.1).

12.3. W. V. Quine, *On the theory of types,* JSL, vol. 3 (1938), pp. 125-139.

12.4. Alonzo Church, *A formulation of the simple theory of types,* JSL, vol. 5 (1940), pp. 56-68.

12.5. Leon Henkin, *Completeness in the theory of types,* JSL, vol. 15 (1950), pp. 81-91.

Also 1.3, 1.27, 1.28, 5.18, 5.19, 6.7, 6.11, 6.13, 6.15, 6.16, 6.17, 13.1, 21.3, 24.2.

13. GÖDEL'S INCOMPLETENESS THEOREM.

13.1. Kurt Gödel, *Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme,* MMP, vol. 38 (1931), pp. 173-198.

13.2. Barkley Rosser, *Extensions of some theorems of Gödel and Church,* JSL, vol. 1 (1936), pp. 87-91.

13.3. Barkley Rosser, *An informal exposition of proofs of Gödel's theorems and Church's theorem,* JSL, vol. 4 (1939), pp. 53-60. A non-technical sketch with deliberate sacrifice of accuracy in favor of (rough) intelligibility.

Also 6.10 (an informal sketch), 6.13, 6.14, 6.16, 6.18, 14.1, 14.6, 21.7 (brief semi-formal outline).

14. RECURSIVE FUNCTIONS AND RELATED TOPICS.

14.1. S. C. Kleene, *General recursive functions of natural numbers,* Mathematische Annalen, vol. 112 (1936), pp. 727-742.

14.2. Alonzo Church, *An unsolvable problem of elementary number theory,* AJM, vol. 58 (1936), pp. 345-363.

14.3. A. M. Turing, *On computable numbers, with an application to the Entscheidungsproblem,* Proceedings of the London Mathematical Society, ser. 2 vol. 42 (1936-7), pp. 230-265, and vol. 43 (1937), pp. 544-546.

14.4. E. L. Post, *Finite combinatory processes — formulation 1,* JSL, vol. 1 (1936), pp. 103-105.

14.5. A. M. Turing, *Computability and λ-definability,* JSL, vol. 2 (1937), pp. 153-163.

14.6. S. C. Kleene, *Recursive predicates and quantifiers,* Transactions of the American Mathematical Society, vol. 53 (1943), pp. 41-73.

14.7. E. L. Post, *Formal reductions of the general combinatorial decision problem,* AJM, vol. 65 (1943), pp. 197-215.

14.8. E. L. Post, *Recursively enumerable sets of positive integers and their decision problems,* Bulletin of the American Mathematical Society, vol. 50 (1944), pp. 284-316.

14.9. S. C. Kleene, *On the interpretation of intuitionistic number theory,* JSL, vol. 10 (1945), pp. 109-124.

14.10. David Nelson, *Recursive functions and intuitionistic number theory,* Transactions of the American Mathematical Society, vol. 61 (1947), pp. 307-368, 556.

14.11. Andrzej Mostowski, *On definable sets of positive integers,* Fundamenta Mathematicae,[7] vol. 34 (1947), pp. 81-112.

14.12. Andrzej Mostowski, *On a set of integers not definable by means of one-quantifier predicates,* Annales de la Société Polonaise de Mathématique, vol. 21 (1948), pp. 114-119.

14.13. Rózsa Péter, *Rekursive Funktionen,* Budapest 1951, 206 pp.

14.14. S. C. Kleene, *Introduction to Metamathematics,* forthcoming book.

Also 6.18, 24.2, 25.1. See further 10.

15. FOUNDATIONS OF ARITHMETIC.

15.1. Thoralf Skolem, *Begründung der elementaren Arithmetik durch die rekurrierende Denkweise ohne Anwendung scheinbarer Veränderlichen mit unendlichem Ausdehnungsbereich,* Skrifter Utgift av Videnskapsselskapet i Kristiania, 1923, no. 6, 38 pp.

15.2. Jacques Herbrand, *Sur la non-contradiction de l'arithmétique,* Journal für die Reine und Angewandte Mathematik, vol. 166 (1931-2), pp. 1-8.

15.3. Thoralf Skolem, *Über die Nicht-charakterisierbarkeit der Zahlenreihe mittels endlich oder abzählbar unendlich vieler Aussagen mit ausschliesslich Zahlenvariablen,* FM, vol. 23 (1934), pp. 150-161. (See also 20.1, 20.2.)

15.4. Wilhelm Ackermann, *Zur Widerspruchsfreiheit der Zahlentheorie,* Mathematische Annalen, vol. 117 (1940), pp. 162-194.

Also 5.8, 5.9, 5.10, 5.12, 5.13, 5.15, 5.16, 5.19, 6.15, 6.18, 17.2, 21.1.

16. INFORMAL AXIOMATIC METHOD IN THE FOUNDATIONS OF MATHEMATICS.

16.1. J. W. Young, *Lectures on Fundamental Concepts of Algebra and Geometry,* New York 1911, vii + 247 pp.; reprinted New York 1916.

Also 1.3, 6.17 (new edition).

17. INFORMAL SET THEORY.

17.1. Bernard Bolzano, *Paradoxien des Unendlichen,* Leipzig 1851; reprinted Leipzig 1920, with commentary by Hans Hahn. *Paradoxes of the Infinite,* English translation (with historical introduction) by D. A. Steele, London and New Haven 1950, ix + 189 pp.

17.2. Richard Dedekind, *Was Sind und Was Sollen die Zahlen?,* Braunschweig 1888, xv + 58 pp.; 2nd edn., 1893; 3rd edn., 1911; 6th edn., 1930; reprinted in *Gesammelte Mathematische Werke,* vol. 3 (1932), pp. 335-391. English translation by W. W. Beman in *Essays on the Theory of Numbers,* by Richard Dedekind, Chicago and London 1901; 2nd edn., Chicago 1909.

17.3. Georg Cantor, *Beiträge zur Begründung der transfiniten Mengenlehre,* Mathematische Annalen, vol. 46 (1895), pp. 481-512, and vol. 49 (1897), pp. 207-246; reprinted in 17.4. *Contributions to the Founding of the Theory of Transfinite Numbers,* English translation (with introduction and notes) by P. E. B. Jourdain, Chicago and London 1915.

17.4. Georg Cantor, *Gesammelte Abhandlungen,* edited and with notes by Ernst Zermelo, and with a biography by A. Fraenkel, Berlin 1932, vii + 486 pp.

17.5. Wacław Sierpiński, *Leçons sur les Nombres Transfinis,* Paris 1928, vi + 240 pp.; reprinted Paris 1950.

18. AXIOMATIC SET THEORY.

18.1. A. Fraenkel, *Einleitung in die Mengenlehre,* 3rd edn. (revised and enlarged), Berlin 1928, xiii + 424 pp. Revised English translation forthcoming.

18.2. Alonzo Church, *Zermelo set theory,* The Dictionary of Philosophy, edited by D. D. Runes, New York 1942, pp. 180-181. Convenient source of a brief statement of the Zermelo-Skolem form of axiomatic set theory.

18.3. Paul Bernays, *A system of axiomatic set theory,* JSL, vol. 2 (1937), pp. 65-77, vol. 6 (1941), pp. 1-17, vol. 7 (1942), pp. 65-89, 133-145, vol. 8 (1943), pp. 89-106, vol. 13 (1948), pp. 65-79. Best exposition of the von Neumann-Bernays-Gödel form of axiomatic set theory.

18.4. Kurt Gödel, *The Consistency of the Axiom of Choice and of the Generalized Continuum-Hypothesis with the Axioms of Set Theory,* Princeton 1940, 66 pp.; second printing with minor corrections and additions, 1951.

18.5. Hao Wang, *Remarks on the comparison of axiom systems,* Proceedings of the National Academy of Sciences of the U. S. A., vol. 36 (1950), pp. 448-453. See review by Andrzej Mostowski in JSL, vol. 16 (1951), pp. 142-143.

18.6. Andrzej Mostowski, *Some impredicative definitions in the axiomatic set-theory,* FM, vol. 37 (1950), pp. 111-124.

Also 5.17, 20.1, 20.4, 31.3.

19. AXIOM OF CHOICE.

19.1. Alonzo Church, *Alternatives to Zermelo's assumption,* Transactions of the American Mathematical Society, vol. 29 (1927), pp. 178-208.

19.2. Adolf Lindenbaum and Andrzej Mostowski, *Über die Unabhängigkeit des Auswahlaxioms und einiger seiner Folgerungen,* CRV, vol. 31 (1938), pp. 27-32.

19.3. Andrzej Mostowski, *Über die Unabhängigkeit des Wohlordnungssatzes vom Ordnungsprinzip,* FM, vol. 32 (1939), pp. 201-252.

19.4. Henri Lebesgue, *Les controverses sur la théorie des ensembles et la question des fondements,* EdZ, pp. 109-122.

19.5. Wacław Sierpiński, *L'axiome du choix et l'hypothèse du continu,* EdZ, pp. 125-143.

Also 5.17, 6.17, 18.1, 18.3, 18.4, 21.1.

20. THE SKOLEM PARADOX AND THE RELATIVITY OF THE CONCEPTS OF SET THEORY.

20.1. Thoralf Skolem, *Sur la portée du théorème de Löwenheim-Skolem,* EdZ, pp. 25-52.

20.2. Andrzej Mostowski, *On absolute properties of relations,* JSL, vol. 12 (1947), pp. 33-42.

20.3. G. Kreisel, *Note on arithmetic models for consistent formulae of the predicate calculus,* FM, vol. 37 (1950), pp. 265-285.

20.4. Roman Suszko, *Canonic axiomatic systems,* Studia Philosophica (Poznań), vol. 4 (1951), pp. 301-330.

Also 18.1.

21. PHILOSOPHY OF MATHEMATICS.

21.1. Bertrand Russell, *Introduction to Mathematical Philosophy,* London 1919, viii + 206 pp.; 2nd edn., London 1920, reprinted London 1924, 1930, 1938, 1948.

21.2. David Hilbert, *Die Grundlagen der Mathematik,* with a note by Paul Bernays and a reply on behalf of mathematical intuitionism by Hermann Weyl, Abhandlungen aus dem Mathematischen Seminar der Hamburgischen Universität, vol. 6 (1928), pp. 65-92. Reprinted as no. 5 of Hamburger Mathematische Einzelschriften, Leipzig 1928. Reprinted without the notes by Weyl and Bernays as Anhang IX of the seventh edition of Hilbert's *Grundlagen der Geometrie.* The material should be read in the light of later developments as set forth in 6.18 (in particular the results of Gödel 13.1).

21.3. F. P. Ramsey, *The Foundations of Mathematics and Other Logical Essays,* London and New York 1931, xviii + 292 pp.; reprinted London and New York 1950.

21.4. Max Black, *The Nature of Mathematics,* London 1933, New York 1934, xiv + 219 pp.; reprinted London and New York 1950.

21.5. Kurt Gödel, *Russell's mathematical logic,* The Philosophy of Bertrand Russell, edited by P. A. Schilpp, Evanston and Chicago 1944, pp. 123-153.

21.6. E. W. Beth, *Wijsbegeerte der Wiskunde,* 2nd edn., Antwerp and Nijmegen 1948, 387 pp.

21.7. Hermann Weyl, *Philosophy of Mathematics and Natural Science,* Princeton 1949, x + 311 pp.

See further 22, 23.

22. MATHEMATICAL INTUITIONISM.

22.1. Hermann Weyl, *Das Kontinuum,* Leipzig 1918, iv + 83 pp.; reprinted Leipzig 1932. Not the Brouwerian intuitionism which Weyl later espoused, but a lesser departure from classical mathematical methods, based on rejection of impredicative definition.

22.2. A. N. Kolmogoroff, *Sur le principe de tertium non datur* (Russian), Recueil Mathématique de la Société Mathématique de Moscou, vol. 32 (1924-5), pp. 646-667.

22.3. A. N. Kolmogoroff, *Zur Deutung der intuitionistischen Logik,* Mathematische Zeitschrift, vol. 35 (1932), pp. 58-65.

22.4. KURT GÖDEL, *Zur intuitionistischen Arithmetik und Zahlentheorie*, Ergebnisse eines Mathematischen Kolloquiums, no. 4 (1933), pp. 34-38.

22.5. AREND HEYTING, *Mathematische Grundlagenforschung, Intuitionismus, Beweistheorie*, Ergebnisse der Mathematik und ihrer Grenzgebiete, vol. 3 no. 4, Berlin 1934, iv + 73 pp.

22.6. L. E. J. BROUWER, *Consciousness, philosophy, and mathematics*, XCP, pp. 1235-1249.

22.7. SIGEKATU KURODA, *Intuitionistische Untersuchungen der formalistischen Logik*, Nagoya Mathematical Journal, vol. 2 (1951), pp. 35-47.

Also 6.8, 14.9, 14.10, 18.1, 21.2, 21.4, 21.6, 21.7, 24.1, 24.2, 26.4.

23. SEMI-INTUITIONISM OR EMPIRICISM IN MATHEMATICS.

23.1. PAUL BOCKSTAELE, *Het Intuïtionisme bij de Franse Wiskundigen*, Brussels 1949, 123 pp.

Also 22.5.

24. HILBERTIAN PROOF THEORY.

24.1. PAUL BERNAYS, *Sur les questions méthodologiques actuelles de la théorie hilbertienne de la démonstration*, EdZ, pp. 144-161.

24.2. ARNOLD SCHMIDT, *Mathematische Grundlagenforschung*, Enzyklopädie der Mathematischen Wissenschaften, Band I. Algebra und Zahlentheorie, 1. Teil, Heft 1, Teil II, Leipzig 1950, 48 pp.

Also 6.15, 6.18, 15.2, 15.4, 21.2, 21.6, 21.7, 22.5.

25. COMBINATORY LOGIC, λ-CONVERSION.

25.1. ALONZO CHURCH, *The Calculi of Lambda-Conversion*, Princeton 1941, ii + 77 pp.; second printing 1951 with minor revisions and additions.

25.2. J. B. ROSSER, *New sets of postulates for combinatory logics*, JSL, vol. 7 (1942), pp. 18-27.

25.3. H. B. CURRY, *The combinatory foundations of mathematical logic*, JSL, vol. 7 (1942), pp. 49-64.

25.4. ROBERT FEYS, *La technique de la logique combinatoire*, Revue Philosophique de Louvain, vol. 44 (1946), pp. 74-103, 237-270.

25.5. H. B. CURRY, *A simplification of the theory of combinators*, Synthese, vol. 7 (1948-9), pp. 391-399.

Also 6.8, 6.9, 14.2, 14.5, 14.6.

26. NATURAL INFERENCE, SEQUENZENKALKÜL.

26.1. Stanisław Jaśkowski, *On the Rules of Suppositions in Formal Logic,* Warsaw 1934, 32 pp.

26.2. Gerhard Gentzen, *Untersuchungen über das logische Schliessen,* Mathematische Zeitschrift, vol. 39 (1934-5), pp. 176-210, 405-431.

26.3. Oiva Ketonen, *Untersuchungen zum Prädikatenkalkül,* Annales Academiae Scientiarum Fennicae, series A, I. Mathematica-Physica 23, Helsinki 1944, 71 pp.

26.4. H. B. Curry, *A Theory of Formal Deducibility,* Notre Dame 1950, ix + 126 pp.

26.5. Kurt Schütte, *Schlussweisen-Kalküle der Prädikatenlogik,* Mathematische Annalen, vol. 122 (1950), pp. 47-65.

26.6. W. V. Quine, *On natural deduction,* JSL, vol. 15 (1950), pp. 93-102.

Also 6.4, 6.9.

27. ALGEBRA OF CLASSES, BOOLEAN ALGEBRA.

27.1. E. V. Huntington, *Sets of independent postulates for the algebra of logic,* Transactions of the American Mathematical Society, vol. 5 (1904), pp. 288-309.

27.2. Alfred Tarski, *Einige Bemerkungen zur Axiomatik der Boole'schen Algebra,* CRV, vol. 31 (1938), pp. 33-35.

Also 1.20, 5.7, 5.11, 6.1, 6.16, 30.1.

28. ALGEBRA OF RELATIONS.

28.1. Alfred Tarski, *On the calculus of relations,* JSL, vol. 6 (1941), pp. 73-89.

Also 5.6, 5.11, 6.1.

29. MANY-VALUED LOGIC.

29.1. Jan Łukasiewicz, *Philosophische Bemerkungen zu mehrwertigen Systemen des Aussagenkalküls,* CRV, vol. 23 (1930), pp. 51-77.

29.2. J. B. Rosser and A. R. Turquette, papers in JSL, vol. 10 (1945), pp. 61-82, vol. 13 (1948), pp. 177-192, vol. 14 (1949), pp. 219-225, vol. 16 (1951), pp. 22-34.

Also 3.7, 6.8, 6.16, 7.1, 7.3, 7.4, 7.7, 30.1.

30. MODALITY, STRICT IMPLICATION.

30.1. C. I. Lewis and C. H. Langford, *Symbolic Logic,* New York 1932, xi + 506 pp.; reprinted New York 1951.

30.2. Kurt Gödel, *Eine Interpretation des intuitionistischen Aussagenkalküls,* Ergebnisse eines Mathematischen Kolloquiums, no. 4 (1933), pp. 39-40.

30.3. W. T. Parry, *Modalities in the Survey system of strict implication*, JSL, vol. 4 (1939), pp. 137-154.

30.4. J. C. C. McKinsey, *A solution of the decision problem for the Lewis systems S2 and S4, with an application to topology*, JSL, vol. 6 (1941), pp. 117-134.

30.5. Rudolf Carnap, *Modalities and quantification*, JSL, vol. 11 (1946), pp. 33-64.

30.6. Ruth C. Barcan, papers in JSL, vol. 11 (1946), pp. 1-16, 115-118, vol. 12 (1947), pp. 12-15.

30.7. J. C. C. McKinsey and Alfred Tarski, *Some theorems about the sentential calculi of Lewis and Heyting*, JSL, vol. 13 (1948), pp. 1-15.

30.8. F. B. Fitch, *Intuitionistic modal logic with quantifiers*, Portugaliae Mathematica, vol. 7 no. 2 (1949), pp. 113-118.

30.9. Sören Halldén, *Några Resultat i Modal Logik* (Dissertation Uppsala 1950), 34 pp.

30.10. Arnold Schmidt, *Systematische Basisreduktion der Modalitäten bei Idempotenz der positiven Grundmodalitäten*, Mathematische Annalen, vol. 122 (1950), pp. 71-89.

30.11. C. I. Lewis, *Notes on the logic of intension*, Structure, Method and Meaning, Essays in Honor of Henry M. Sheffer, New York 1951, pp. 25-34.

Also 1.8, 1.20, 6.8, 6.10, 26.4, 32.6, 33.4, 33.9.

31. MODELS OF LOGICAL SYSTEMS.

31.1. J. G. Kemeny, *Models of logical systems*, JSL, vol. 13 (1948), pp. 16-30.

31.2. J. B. Rosser and Hao Wang, *Non-standard models for formal logics*, JSL, vol. 15 (1950), pp. 113-129.

31.3. I. L. Novak, *A construction for models of consistent systems*, FM, vol. 37 (1950), pp. 87-110. Cf. 18.5, 18.6.

Also 12.5.

32. SEMANTICS.

32.1. Alfred Tarski, *Der Wahrheitsbegriff in den formalisierten Sprachen*, Studia Philosophica, vol. 1 (1936), pp. 261-405.

32.2. Alfred Tarski, *Grundlegung der wissenschaftlichen Semantik*, Actes du Congrès International de Philosophie Scientifique, Paris 1936, part III, pp. 1-8.

32.3. Alfred Tarski, *Über den Begriff der logischen Folgerung*, ibid., part VII, pp. 1-11.

32.4. Rudolf Carnap, *Introduction to Semantics*, Cambridge, Mass., 1942, xii + 263 pp.

32.5. Alfred Tarski, *The semantic conception of truth and the foundations of semantics*, Philosophy and Phenomenological Research, vol. 4 (1943-4), pp. 341-376; reprinted in *Readings in*

Philosophical Analysis, edited by Herbert Feigl and Wilfrid Sellars, New York 1949, pp. 52-84.

32.6. Rudolf Carnap, *Meaning and Necessity,* Chicago 1947, viii + 210 pp.

32.7. J. C. C. McKinsey, *A new definition of truth,* Synthese, vol. 7 (1948-9), pp. 428-433.

32.8. W. V. Quine, Max Black, Alonzo Church, Part 3 of *Contributions to the Analysis and Synthesis of Knowledge,* Proceedings of the American Academy of Arts and Sciences, vol. 80 no. 1 (1951), pp. 89-112.

33. SENSE AND DENOTATION, AND RELATED DISTINCTIONS OF MODES OF MEANING.

33.1. Gottlob Frege, *Über Sinn und Bedeutung,* Zeitschrift für Philosophie und Philosophische Kritik, vol. 100 (1892), pp. 25-50. *Sense and reference,* English translation by Max Black, The Philosophical Review, vol. 57 (1948), pp. 209-230. *On sense and nominatum,* English translation by Herbert Feigl in *Readings in Philosophical Analysis,* New York 1949, pp. 85-102.

33.2. Bertrand Russell, Appendix A of 5.16.

33.3. Bertrand Russell, *On denoting,* Mind, vol. 14 (1905), pp. 479-493; reprinted in *Readings in Philosophical Analysis,* New York 1949, pp. 103-115.

33.4. W. V. Quine, *Notes on existence and necessity,* The Journal of Philosophy, vol. 40 (1943), pp. 113-127.

33.5. Alonzo Church, review of 32.4, The Philosophical Review, vol. 52 (1943), pp. 298-304.

33.6. C. I. Lewis, *The modes of meaning,* Philosophy and Phenomenological Research, vol. 4 (1943-4), pp. 236-250.

33.7. C. I. Lewis, *An Analysis of Knowledge and Valuation,* La Salle, Ill., 1946, xxi + 567 pp.

33.8. W. V. Quine, *Two dogmas of empiricism,* The Philosophical Review, vol. 60 (1951), pp. 20-43.

33.9. Alonzo Church, *A formulation of the logic of sense and denotation,* Structure, Method and Meaning, Essays in Honor of Henry M. Sheffer, New York 1951, pp. 3-24.

Also 4.1, 4.2, 5.13, 6.10, 32.6, 32.8.

---

[1] These Proceedings will be referred to below (throughout this bibliography) as "XCP."

[2] This periodical will be referred to below as "JSL."

[3] This periodical will be referred to below as "AJM."

[4] Class III of this periodical will be referred to below as "CRV."

[5] This book will be referred to below as "EdZ."

[6] This periodical will be referred to below as "MMP."

[7] This periodical will be referred to below as "FM."

SOME SIGNIFICANT TRENDS TOWARD INTEGRATION IN THE SCIENCES OF MAN[1]

LAURA THOMPSON

It is generally agreed that the basic assumptions, methodologies and goals of a unified and mature science of man have not yet been formulated systematically in a way to gain general acceptance, much less in a way to be applied to the crucial problems of our time. Without such systematic formulation it is easy to overlook certain convergent discoveries and trends toward integration in the many academically-unrelated disciplines that deal with man, or to ignore their significance. The purpose of this extremely brief bibliography is: 1. to call attention to some trends in the human disciplines which appear to be especially significant regarding the eventual emergence of a systematic, unified science of man; and 2. to illustrate the thesis by means of a limited number of references, especially to recent publications in the English language. It should be emphasized that both thesis and documentation are intended to be merely suggestive and illustrative rather than definitive. Obviously no attempt has been made at complete coverage of the vast relevant literature. It is hoped, however, that even such a brief presentation may help to clarify certain significant trends and to stimulate further research in these directions.

TRENDS TOWARD INTEGRATION IN THEORETICAL APPROACH

1. *The sciences of man are natural sciences.* The biological base of the human disciplines is receiving more and more attention, not only in the biological sciences (Darwin, Geddes and Thomson p. 147f.) including genetics (Dobzhansky, Sinnott et al. 1950), physiology (Cannon, Lillie, Pavlov, p. 395f., Sherrington), physical anthropology (Boyd, Hooton, Montagu), social and cultural ecology, ethnogeography and ethnobotany (Adams 1935, Allee 1938, Carter, Malin, Mumford, Sears 1939), and nutrition (Postmus and VanVeen), but also in the psychological and the social sciences (Cantril 1950, Cameron, Frank 1951, Cameron, Haring, Hebb, Hunt ed., Malinowski, Murphy, Sheldon 1940, 1942, 1949). An organic approach rather than

---

[1] I am indebted to John Collier, Charles Morris and David Bidney for helpful comments on the manuscript.

a "superorganic" or "superpsychic" approach is being stressed (Bidney 1947, 1949b, Mumford p. 300f., Sapir 1917), and the traditional dual division between "natural" and "social" sciences is becoming outmoded (Whyte p. 17). Related to this trend is an increasing interest in cooperative processes in communities and societies (Bishop, Deets, Haskell ed., M. Mead ed.), and a growing conviction that cooperation is a basic principle of organic life (Allee 1938, Kropotkin, Montagu).

2. *The significant unit of research in the sciences of man is a human event in space-time.* Whether the focus be an individual organism or a group of organisms, there is a trend toward viewing the significant unit of research in the human sciences as a multidimensional human event in space-time rather than, e.g., as an object or thing, a "trait" or a pattern, or as an aggregation of objects, "traits", or patterns simply located in an environment as though in a container or mold (Bentley 1926 p. 48f., Frank 1951 p. 31f., Herrick p. 163f., 206f., Whitehead 1926 p. 72f.). From this emerging viewpoint a pattern or a system or a process, whether it be of overt behavioral activities, of institutions, symbols, norms or goals, represents the unknown (e.g., x) to be discovered, and the scientific problem is formulated in terms of its multidimensional exploration (see 3 below). The relevant trend is toward recognizing, as the given significant unit of research, not the pattern or the process itself but rather the patterned or processual event or happening, including its prospective (moving forward into the future) phases and its retrospective (historical) phases.

Whether reference be made to a psychosomatic or a personality unit (Ames, Herrick, Meyer, Murphy), to a local region or community (Malin, Sears 1949), to a society (Lewin 1948), or to a culture (Benedict 1934, Malinowski, Thompson 1950), emphasis is increasingly on the organism-environment event or happening as a complex whole. The "known" and the "knower" are conceived as profoundly intervolved (Ames, Cantril 1950, Dewey and Bentley), and transactional rather than merely reciprocal or interactional relationships are beginning to be stressed (Bentley 1950, Cantril et al. 1949; Frank 1951). Closely related to these developments are the new action-research and operational-research methodologies involving a team composed of scientists from various disciplines, administrators, and laymen working together to solve a practical problem (Bishop, Collier 1945, Dobbs, Lewin 1948, ch. 13, Thompson 1951b, p. 29f.).

3. *Significant problems in the sciences of man deal with the*

inner structural relations of an organized dynamic "field". A significant tendency is to view the multidimensional human event in space-time as a dynamic "field" and to view the scientific problem as the discovery of the structural organization of this "field" (Kluckhohn and Murray ed., Korzybski, Lewin 1951). (For dangers to be avoided in using the "field" approach see Bentley 1950.)

4. *The trend, in terms of the present thesis, is away from investigating a human problem merely from a single viewpoint* (e.g., the "economic", the somatic, the Freudian, the overt behavioral) *and toward perceiving the human event from many viewpoints simultaneously* ("inside" and "outside", "above" and "below", "far" and "near") *so that the relationships between its various aspects and its inner and outer form may be discovered* (see 6 below; also Adams 1935, Bentley 1926 p. 57f., Collier 1947, Giedion, Lee 1950, Malin, Mannheim, Parsons and Shils, Thompson 1950, Thurnwald, Wittfogel and Fêng). One important effect of this trend is that the sex factor is being placed in proper relation to other factors equally important (Allee 1931 p. 4). Furthermore, regarding the psychological dimension, in this type of research the psychiatric approach is beginning to take precedence over the Freudian psychoanalytical approach (Chrisholm, Frank 1948, Hallowell 1950, Hunt ed., A. H. Leighton, M. Mead 1939, Meyer, Sullivan, Thompson 1951b).

The above-mentioned trends reveal, throughout the fundamental sciences of man, a convergence in basic theoretical approach which manifestly is related to recent trends in physics and mathematics (Bentley 1926 pt. 1, Einstein and Infeld, Whitehead 1926), in logic (Dewey 1938, Dewey and Bentley, Kantor), and in modern art and architecture (Giedion, Mumford).

Trends Toward Integrative Multidiscipline Research

5. *The new approach, especially concentration on multidimensional human events in space-time, on transactional relationships, and on perception from many viewpoints simultaneously, together with the imperative need to solve practical problems* (Lynd), *has necessitated the crossing of barriers between traditional academic disciplines dealing with mankind.* Indeed, artificial barriers are breaking down under the clinical test and a shakeup of the human disciplines is going forward. From the shakeup are emerging a number of key dimensions or vantage

points from which to view the human event in historical and areal perspective (looking both ''backward'' and ''forward'' in time and in all directions ''inside'' and ''outside'' in space). These are taking form as the basic integral dimensions of the future unified science of mankind. Although still in the formative stage, for purposes of the present thesis they may be conveniently designated as follows (Frank 1951, Morris 1948 p. 30, Thompson 1950, 1951 ch. 8): *the ecologic dimension*—society-nature transactions; *the somatic dimension*—somatic transactions; *the social dimension*—interpersonal and intergroup transactions; *the psychic dimension*—psychological transactions; *the symbolic dimension*—symbolic and semantic transactions including language, ceremonials, arts and crafts, music, mythology, folklore, sciences, etc. (see Cassirer p. 24f., Morris 1946); *the core values dimension*—basic evaluational transactions (see Frank 1948, p. 365f., Kluckhohn 1949, Murphy p. 815, Parsons and Shils, Thompson 1951b ch. 8). Parenthetically it should be noted that alongside the major divisions outlined above, attempts are being made in the direction of developing a more adequate philosophy of the science of mankind (see, e.g., Bidney 1949a, Cassirer, Dewey 1934, 1938, Frank 1951, Herrick, Lillie, G. H. Mead, Northrop 1947, Smuts, Whitehead 1925, 1933).

6. *Significant attempts are being made to investigate the inner structural relations between two or more of these basic dimensions of the emergent science of mankind.* For example, inquiries into the relations between the following basic dimensions are going forward, to mention only a few illustrations:[2] ecologic and somatic (Monge); ecologic and social (Carter, Polanyi, Firth); ecologic, somatic, social (Postmus and Van-Veen); ecologic, social, symbolic (Giedion, Herskovits, Kroeber, Malin, Mumford); ecologic, social, symbolic, core values (Deets, Kluckhohn and Leighton, Wittfogel and Fêng); ecologic, somatic, social, psychic, symbolic (Hunt ed., Joseph and Murray); ecologic, social, psychic, symbolic, core values (Burckhardt, Hallowell 1950, Thurnwald); ecologic, somatic, social, psychic, symbolic, core values (Collier 1947, Joseph et al., Thompson 1950); somatic and psychic (Hebb, Lillie, Sheldon 1940, 1942, Sinnott); somatic, social, psychic (G. H. Mead, Sheldon 1949); somatic, social, symbolic (Morris 1946, Wiener); somatic, social, psychic, symbolic, core values (A. H. Leighton); social and psychic

---

[2] It should be emphasized that the attempts cited and their categorization are merely suggestive and by no means definitive.

(Moreno, Murdock); social, psychic, symbolic (DuBois, Fromm 1941, Kardiner et al., Leighton and Kluckhohn, Linton, Overstreet, Sapir 1949); social, psychic, symbolic, core values (Benedict 1934, 1946, Frank 1948, Fromm 1949, Jaeger, Lee 1949, 1950); social, symbolic, core values (Malinowski, Morris 1948, Northrop 1946, Whorf); social, psychic, core values (Parsons and Shils); symbolic and core values (Alexander, Korzybski).

These and other attempts have led in the direction of multidiscipline cooperative research in the sciences of man (see 6 above), and to the development of integral depth analysis of complex whole human events (see 6).

7. *The new approach has revealed the need of field methods and laboratory techniques of requisite precision and penetration* (Adams 1935, Freud, Hallowell 1949, Kluckhohn and Murray ed., Lewin 1948, 1951, Malin, Malinowski, Murdock, Radcliffe-Brown, Redfield), and has led to a more critical investigation of the whole perceptive process (Ames, Hebb, Korzybski, Lee 1950, Lewin 1948 p. 56f., Mannheim p. 212f., Murphy ch. 14).

8. *The development of properly precise and penetrating methods and of multidimensional crossdiscipline research has necessitated a thoroughgoing revision of traditional concepts* (Bentley 1926 p. 57f., Bidney 1947, 1949b, Cantril 1950, Dewey and Bentley, Frank 1951, Korzybski, Murphy, Parsons and Shils, Whitehead 1926, 1933).

9. *There is increasing interest in the formative processes* (Adams 1940, Malin, Smuts, Whyte p. 21) *whereby eco-cultural, social, and psychosomatic wholes are integrated, especially in the directiveness of organic activities* (Cannon, Lillie, Russell, Sears 1939, 1949, Sinnott), *the purposiveness of individual, social and cultural processes* (Ames, Bentley 1926, Malinowski p. 52f., Overstreet, Redfield 1941 p. 141f., Tolman), *and in human creativity* (Bergson, Cassirer p. 223f., Morris 1948, Wertheimer). There is increasing recognition that the processes of adaptation and adjustment to the environment cannot of themselves completely account for such wholes (Hallowell 1950, Kluckhohn 1949, Overstreet p. 73), nor can adaptation plus organic integration. Over and above these processes there is being identified a distinctively *human* integrating process, a symbolic process tending toward the formation of balanced, whole, self-regulating personalities and communities (Burke, Cassirer, Jaeger, Malinowski ch. 5). The fundamentally esthetic nature of this process (Alexander, Cantril 1950, Dewey 1934) and also its inherent logic (Sorokin v. 1 p. 18f.) are receiving increasing attention.

10. As an outgrowth of all these developments, *the crucial importance of basic, emotionally-tinged attitudes and core values is being more and more recognized in the life sciences and in practical everyday living problems* (Adams 1940, Ames, Bateson, Benedict 1934, 1946, Bishop, Cantril 1950, Collier 1947, Deets, Frank 1948, 1951, Jaeger, Kluckhohn 1949, Lee 1949, 1950, Morris 1948, Overstreet, Piaget, Sears 1939, Sorokin, Thompson 1950, Whitehead 1933).

Unfortunately, space limitations preclude discussion of how the new integral multidiscipline approach in the sciences of man is being applied to the practical problems of our time. The present writer has dealt briefly with this aspect of the thesis elsewhere (Thompson 1951a).

SELECTED BIBLIOGRAPHY

Adams, Charles C., 1935. "The Relation of General Ecology to Human Ecology," *Ecology* 16:316-35 (contains a useful bibliography).

————, 1940. "Selected References on the Relation of Science to Modern Life," New York State Museum bul. 322: 79-96.

Alexander, Hartley B., 1926. *L'Art et la Philosophie des Indiens de L'Amérique du Nord,* E. Reroux, Paris.

Allee, W. C., 1931. *Animal Aggregations,* U. of Chicago, Chicago.

————, 1938. *The Social Life of Animals,* Norton, New York (presents evidence that a general principle of cooperation is one of the fundamental biological principles).

Ames, Adelbert, Jr., 1949. "Architectural Form and Visual Sensations," in T. H. Creighton (ed.) *Building for Modern Man,* Princeton U., Princeton (contains a summary of Ames' findings on perception).

Bateson, Gregory, 1949. "Bali: The Value System of a Steady State", in M. Fortes (ed.) *Essays presented to A. R. Radcliffe-Brown,* Clarendon, London.

Benedict, Ruth, 1934. *Patterns of Culture,* Houghton Mifflin, Boston.

————, 1946. *The Chrysanthemum and the Sword: Patterns of Japanese Culture,* Houghton Mifflin, Boston (probably the best study of "national character" by an anthropologist yet to appear).

Bentley, Arthur F., 1926. *Relativity in Man and Society,* Putnam, New York. (One of the few attempts to translate Einsteinian relativity into terms relevant to the social scientist. The section on application is limited by lack of full consideration of the space (ecological) dimension.)

————, 1950. "Kinetic Inquiry," *Science* 112: 775-83. (A brief summary of progress to date in organized investigation into the problem of human "knowings" and "knowns" where these form a common field.)

Bergson, Henri, 1946. *The Creative Mind* (M. L. Andison, tr.), Philosophical Library, New York.

Bidney, David, 1947. "Human Nature and the Cultural Process," *American Anthropologist* 49: 375-99.

————, 1949a. "The Concept of Meta-Anthropology and Its Significance for Contemporary Anthropological Science," in F. S. C. Northrop's (ed.) *Ideological Differences and World Order,* Yale U. Press, New Haven.

————, 1949b. "Towards a Psychocultural Definition of the Concept of Personality," in Sargent and Smith's (ed.) *Culture and Personality,* Viking Fund, New York.

Bishop, Claire H., 1950. *All Things Common,* Harper, New York (an account of the European "communities of work" which attempt to find a practical answer to the search for a way of life integrating technological development toward fulfillment of the complete human being).

Boyd, W. C., 1950. *Genetics and the Races of Man. An Introduction to Modern Physical Anthropology,* Little Brown, Boston.

Burckhardt, Jakob, 1929. *The Civilization of the Renaissance in Italy* (H. G. C. Middlemore, tr.), Harper, New York (this classic showed for the first time how a cultural event, the Italian Renaissance, could be treated as a whole, from the viewpoint of its painting, sculpture, architecture and its social institutions).

Burke, Kenneth, 1950. *A Rhetoric of Motives,* Prentice-Hall, New York.

Cameron, N. and Margaret A., 1951. *Behavior Pathology,* Houghton Mifflin, New York.

Cannon, W. B., 1932. *The Wisdom of the Body,* Norton, New York.

Cantril, Hadley, 1950. *The 'Why of Man' Experience,* Macmillan, New York (a brief, systematic formulation of the new transactional psychology).

Cantril, H., A. Ames, Jr., A. H. Hastrof, W. H. Ittelson, 1949. "Psychology and Scientific Research," *Science* 110: 461-64; 491-97; 517-22 (probably the most significant brief formulation on the subject. Stresses the importance of problem posing, discovery of relevant variables, and the role of value judgments in scientific research).

Carter, George, 1950. "Ecology—Geography—Ethnobotany," *Scientific Monthly* 50: 73-80.

Cassirer, Ernst, 1944. *An Essay on Man: An Introduction to a Philosophy of Human Culture,* Yale U., New Haven.

Chrisholm, G. B., 1949. "Social Responsibility," *Science* 109: 27-30, 43.

Collier, John, 1945. "The United States Indian Administration as a Laboratory of Ethnic Relations," *Social Research* 12: 265-303.

————, 1947. *The Indians of the Americas,* Norton, New York (an adequate analysis of Indian-white relations through the centuries and in the present).

Darwin, Charles, 1871. *Descent of Man, and Selection in Relation to Sex,* J. Murray, London, 2 v.

Deets, Lee Emerson, 1939. *The Hutterites: A Study in Social Cohesion.* Ph.D. Thesis, Columbia U., Gettysburg, Pa. (privately printed).

Dewey, John, 1934. *Art as Experience,* Milton Balch, New York.

————, 1938. *Logic, the Theory of Inquiry,* Henry Holt, New York.

Dewey, J. and A. F. Bentley, 1949. *Knowing and the Known.* Beacon Press, Boston (an experiment in cooperative research employing the transactional viewpoint and frame of reference).

Dobbs, H. A. C., 1947. *Operational Research and Action Research* (intro. by J. Collier), Institute of Ethnic Affairs, Washington.

Dobzhansky, T., 1950. "Heredity, Environment and Evolution," *Science* 111: 161-66.

DuBois, Cora, 1944. *The People of Alor: A Social-Psychological Study of an East Indian Island,* U. of Minnesota, Minneapolis.

Einstein, A. and L. Infeld, 1938. *The Evolution of Physics. The Growth of Ideas from Early Concepts to Relativity and Quanta,* Simon and Schuster, New York (comprehensible to the layman).

Frank, Lawrence K., 1948. *Society as the Patient: Essays on Culture and Personality,* Rutgers U., New Brunswick (a balanced approach to the "culture-Personality" problem).

————, 1951. *Nature and Human Nature. Man's New Image of Himself,* Rutgers U., New Brunswick (probably the most adequate statement on the subject yet to appear).

Freud, Sigmund, 1938. *The Basic Writings of Sigmund Freud* (A. A. Brill, tr., ed.), Modern Library, New York.

Fromm, Erich, 1941. *Escape from Freedom,* Farrar and Rinehart, New York.

————, 1947. *Man for Himself: An Inquiry into the Psychology of Ethics,* Rinehart, New York.

Geddes, Patrick and J. A. Thomson, 1925. *Biology,* Henry Holt, New York. (A chart of the relations between biology and the social sciences and humanities.)

Giedion, Siegfried, 1949. *Space, Time and Architecture: The Growth of a New Tradition,* Harvard U., Cambridge (an excellent introduction to modern architecture and art).

Hallowell, A. Irving, 1949. "Ojibwa Personality and Acculturation," *29th International Congress of Americanists,* Sept., New York.

————, 1950. "Values, Acculturation and Mental Health," *American J. of Orthopsychiatry* 20: 732-43.

Haring, Douglas G., 1950. "The Social Sciences and Biology," in *Beiträge zur Gesellungsund Volkerwissenschaft,* Gebr. Mann, Berlin (a discussion of certain aspects of biological science pertinent for the social sciences).

Haskell, Edward (ed.), forthcoming. *An Introduction to Unified Science,* Harper, New York (a collection of essays mostly presented at the Symposium on Cooperation and Conflict among Living Organisms, Sept. 1949, Washington).

Hebb, D. O., 1949. *Organization of Behavior: A Neuropsychological Theory,* Wiley, New York (presents a theory of behavior based, as far as possible, on the physiology of the nervous system).

Herrick, C. Judson, 1949. *George Ellett Coghill: Naturalist and Philosopher,* U. of Chicago, Chicago.

Herskovits, Melville J., 1940. *The Economic Life of Primitive Peoples,* Knopf, New York (primitive economics as an introduction to the study of comparative socio-economic systems).

Hooton, Earnest A., 1931. *Up from the Ape,* Macmillan, New York.

Hunt, Joseph McVicker (ed.), 1944. *Personality and the Behavior Disorders,* Ronald Press, New York, 2 v.

Jaeger, Werner, 1943-45. *Paideia: The Ideals of Greek Culture* (G. Highet, tr.), Oxford U., New York, 3 v. (explains the interrelations between the historical process whereby Greek character was formed and the intellectual process whereby the Greeks constructed their ideal of human personality).

Joseph, Alice and Veronica Murray (in press). *The Chamorros and Carolinians of Saipan,* Harvard U., Cambridge.

Joseph, A., R. B. Spicer and J. Chesky, 1949. *The Desert People. A Study of the Papago Indians of Southern Arizona,* U. of Chicago, Chicago.

Kantor, J. R., 1945. *Psychology and Logic,* The Principia Press, Bloomington, Indiana.

Kardiner, Abram, et al., 1945. *The Psychological Frontiers of Society* (intro. by R. Linton), Columbia U., New York (an attempt to apply Freudian psychodynamics to the problem of relating personality and culture).

Kluckhohn, Clyde, 1943. ''Covert Culture and Administrative Problems,'' *American Anthropologist* 45: 213-27.

————, 1949. ''The Limitations of Adaptation and Adjustment as Concepts for Understanding Cultural Behavior,'' in *Adaptation* (J. Romano, ed.), Cornell U., Ithaca (a critique of ''functionalism'' in anthropology).

Kluckhohn, Clyde, and Dorothea C. Leighton, 1946. *The Navaho,* Harvard U., Cambridge (an attempt to describe a culture as a whole).

Kluckhohn, Clyde, and H. A. Murray (ed.), 1948. *Personality in Nature, Society, and Culture* (a valuable collection of essays in the ''personality-culture'' field, except that the ecologic factor is virtually omitted).

Korzybski, Alfred, 1941. *Science and Sanity: An Introduction to Non-Aristotelian Systems and General Semantics,* Non-Aristotelian Library, Lakeville, Conn. 2nd. ed.

Kroeber, A. L., 1939. *Cultural and Natural Areas of Native North America,* U. of California, Berkeley.

Kropotkin, P., 1902. *Mutual Aid: A Factor of Evolution,* Heinemann, London (rev. ed.).

Lee, Dorothy D., 1949. ''Being and Value in a Primitive Culture,'' *J. of Philosophy* 13: 401-11.

————, 1950. ''Lineal and Non-Lineal Codifications of Reality,'' *Psychosomatic Medicine,* 12: 89-97.

Leighton, A. H., 1945. *The Governing of Men: General Principles and Recommendations based on Experience at a Japanese Relocation Camp,* Princeton U., Princeton.

Leighton, Dorothea C., and Clyde Kluckhohn, 1947. *Children of the People: The Navaho Individual and His Development,* Harvard U., Cambridge.

Lewin, Kurt, 1948. *Resolving Social Conflicts: Selected Papers on Group Dynamics* (intro. by W. W. Allport, G. W. Lewin, ed.), Harper, New York.

————, 1951. *Field Theory in Social Science* (D. Cartwright, ed.), Harper, New York (a formulation of Lewin's theory and method of research).

Lillie, Ralph S., 1945. *General Biology and Philosophy of Organism,* U. of Chicago, Chicago.

Linton, Ralph, 1945. *The Cultural Background of Personality,* Appleton-Century, New York.

Lynd, Robert A., 1939. *Knowledge for What?* Princeton U., Princeton.

Malin, James C., 1948. *The Grassland of North America: Prologomena to Its History,* privately multigraphed by Malin, 1541 University Dr., Lawrence, Kansas (a comprehensive regional study with bibliography, from the viewpoints of plant, animal and human ecology, geology, geography, soil science, microbiology and genetics, in historical and philosophical perspective).

Malinowski, Bronislaw, 1944. *A Scientific Theory of Culture and Other Essays,* U. of North Carolina, Chapel Hill (a statement of Malinowski's theoretical position).

Mannheim, Karl, 1940. *Man and Society in an Age of Reconstruction: Studies in Modern Social Structure,* Harcourt Brace, New York (a critique of behaviorism and an exposition of the social planning problem as conceptualized before the discovery of the relevance of ecologic and action-research approaches).

Mead, G. H., 1934. *Mind, Self, and Society from the Standpoint of a Social Behaviorist* (C. W. Morris, ed.), U. of Chicago, Chicago.

Mead, Margaret, 1939. *From the South Seas: Studies of Adolescence and Sex in Primitive Societies,* Morrow, New York.

Mead, Margaret (ed.), 1937. *Cooperation and Competition Among Primitive Peoples,* McGraw-Hill, New York.

Meyer, Adolf, 1944. "The Rise of the Person and the Concept of Wholes or Integration," *American J. of Psychiatry,* 100: 100-17.

Monge, Carlos, 1948. *Acclimatization in the Andes: Historical Confirmations of Climatic Aggression in the Development of Andean Man* (tr. by D. F. Brown, intro. by I. Bowman), Johns Hopkins, Baltimore.

Montagu, Ashley, 1950. *On Being Human,* Schuman, New York.

Moreno, J. L., 1934. *Who Shall Survive: A New Approach to the Problem of Human Interrelations,* Nervous and Mental Disease Pubs. Washington.

Morris, Charles, 1946. *Signs, Language and Behavior,* Prentice-Hall, New York (an attempt to lay the foundations for a comprehensive science of signs by means of a broad behavioral approach).

———, 1948. *The Open Self,* Prentice-Hall, New York.

Mumford, Lewis, 1938. *The Cultures of Cities,* Harcourt Brace, New York.

Murdock, G. P., 1949. *Social Structure,* Macmillan, New York (a behavioristic approach).

Murphy, Gardner, 1947. *Personality: A Biosocial Approach to Origins and Structure,* Harper, New York (a critical review of the literature and a systematic formulation of relevant concepts and generalizations emerging therefrom in broad biosocial frame).

Northrop, F. S. C., 1946. *The Meeting of East and West,* Macmillan, New York.

———, 1947. *The Logic of the Sciences and the Humanities,* Macmillan, New York.

Overstreet, H. A., 1949. *The Mature Mind,* Norton, New York.

Parsons, Talcott; E. A. Shils; and others (forthcoming). *Toward a General Theory of Action,* Harvard U., Cambridge.

Pavlov, I. P., 1927. *Conditioned Reflexes: An Investigation of the Physiological Activity of the Cerebral Cortex* (G. V. Anrep, tr. and ed.), Oxford U., London.

Piaget, Jean, 1929. *The Child's Conception of the World,* Harcourt Brace, New York.

Polanyi, Karl, 1944. *The Great Transformation,* Farrar and Rinehart, New York.

Postmus, A. and A. G. VanVeen, 1949. "Dietary Surveys in Java and East Indonesia," Organization for Scientific Research in Indonesia pub. 1, *Chronica Naturae* 105:229-36, 262-68, 316-23, Batavia (summary of a series of modern diet studies of native communities. State of nutrition is related to state of health in broad context including socio-economic, climatic, soil anthropometric and demographic factors).

Radcliffe-Brown, A., 1922. *The Andaman Islanders,* University Press, Cambridge.

Redfield, Robert, 1941. *Folk Culture of the Yucatan,* U. of Chicago, Chicago.

Russell, E. S., 1945. *The Directiveness of Organic Activities,* University Press, Cambridge (a series of case studies).

Sapir, Edward, 1917. "Do We Need a 'Superorganic'?" *American Anthropologist* 19: 441-47.

———, 1949. *Selected Writings of Edward Sapir in Language, Culture and Personality* (D. G. Mandelbaum, ed.), U. of California, Berkeley.

Sears, Paul B., 1939. *Life and Environment: The Interrelations of Living Things.* Teachers College: Columbia U., New York (written for the layman).

————, 1949. "Integration at the Community Level," *American Scientist* 37: 235-42.

Sheldon, William H., 1940. *The Varieties of Human Physique: An Introduction to Constitutional Psychology,* Harper, New York (a systematic schema for describing and classifying human beings at the morphology level for use in studying personality).

————, 1942. *The Varieties of Temperament: A Psychology of Constitutional Differences,* Harper, New York.

————, 1949. *Varieties of Delinquent Youth: An Introduction to Constitutional Psychiatry,* Harper, New York.

Sherrington, Charles, 1941. *Man on His Nature,* Macmillan, New York (useful for the facts presented).

Sinnott, Edmund W., 1950. *Cell and Psyche: The Biology of Purpose,* U. of North Carolina, Chapel Hill.

Sinnott, E. W., L. C. Dunn and T. Dobzhansky, 1950. *Principles of Genetics,* McGraw-Hill, New York, 4th ed.

Smuts, Jan, 1926. *Holism and Evolution,* Macmillan, New York.

Sorokin, Pitirim A., 1937-41. *Social and Cultural Dynamics,* American Book, New York, 4 v.

Sullivan, Harry Stack, 1940. "Conceptions of Modern Psychiatry", *Psychiatry,* 3: 1-117.

Thompson, Laura, 1950. *Culture in Crisis: A Study of the Hopi Indians* (intro. by J. Collier), Harper, New York (an attempt to understand a whole culture as a multidimensional human event in space-time).

————, 1951a. "Operational Anthropolgy as an Emergent Discipline," Etc.: *A Review of General Semantics,* 8: 117-28.

————, 1951b. *Personality and Government: Findings and Recommendations of the Indian Administration Research,* Institute Indigenista Interamericano, Mexico, D. F. (final report of the Indian Personality and Administration Project).

Thurnwald, Richard C., 1935. *Black and White in East Africa: The Fabric of a New Civilization,* Routledge, London.

Tolman, Edward C., 1932. *Purposive Behavior in Animals and Men,* Century, New York (a behavioristic approach).

Wertheimer, Max, 1945. *Productive Thinking,* Harper, New York.

Whitehead, A. N., 1925. *Science and the Modern World,* Macmillan, New York.

————, 1933. *Adventures of Ideas,* Macmillan, New York.

Whorf, Benjamin Lee, 1941. "The Relation of Habitual Thought and Behavior to Language," in *Language, Culture and Personality, Essays in Memory of Edward Sapir,* Menasha, Wisconsin.

Whyte, L. L., 1948. *The Next Development in Man,* Henry Holt, New York.

Wiener, Norbert, 1948. *Cybernetics: or Control and Communication in the Animal and the Machine,* Wiley, New York.

Wirth, Louis, 1945. "Human Ecology," *American J. of Sociology,* 50: 483-88.

Wittfogel, Karl A. and Fêng, Chia-Shêng, 1949. *History of Chinese Society: Liao (907-1125),* American Philosophical Society, Macmillan, New York (a social history of the Liao empire from a dynamic eco-cultural viewpoint).

Proceedings of the American Academy of Arts and Sciences

Vol. 80, No. 3, P. 187-270, March, 1953

CONTRIBUTIONS TO THE ANALYSIS AND SYNTHESIS OF KNOWLEDGE

3

Published in cooperation with the
Institute for the Unity of Science

CONTENTS

PAGE

Inductive Logic and Science — R. Carnap 189

On Mental Entities — W. V. Quine 198

A Bibliography of Cybernetics —
F. D. Barrett and H. A. Shepard 204

Significs, Its Tendency, Methodology and Applications —
D. Vuysje 223

INDUCTIVE LOGIC AND SCIENCE

RUDOLF CARNAP[1]

The question of the usefulness of inductive logic for science and even the question of the very possibility of inductive logic are today still debated. The conception here explained differs from that of many other scholars.

First I wish to emphasize that inductive logic does not propose *new* ways of thinking, but merely to explicate *old* ways. It tries to make explicit certain forms of reasoning which implicitly or instinctively have always been applied both in everyday life and in science. This is analogous to the situation at the beginning of deductive logic. Aristotle did not invent deductive reasoning; that had gone on as long ago as there was human language. If somebody had said to Aristotle: "What good is your new theory to us? We have done well enough without it. Why should we change our ways of thinking and accept your new invention?", he might have answered: "I do not propose new ways of thinking, I merely want to help you to do consciously and hence with greater clarity and safety from pitfalls what you have always done. I merely want to replace common sense by exact rules."

It is the same with inductive logic. Inductive reasoning is likewise as old as human language. I mean here by inductive reasoning all forms of reasoning or inference where the conclusion goes beyond the content of the premises, and therefore cannot be stated with certainty.

Thus, for example, if a physicist states a new law or a theory as a system of laws on the basis of the experimental results he has found, he makes an inductive inference. So does a scientist who assumes an unknown single fact on the basis of known facts; for example, the meteorologist who predicts the weather for tomorrow, the physicist who assumes a certain distribution of the velocities of gas molecules which he cannot directly observe, a historian who tries to explain a reported act of Abraham Lincoln by hypothetically assuming a certain motivation which is not reported, or a statistician who makes an estimate for the unknown value of a parameter in a population on the basis of an observed sample from the population. Since inductive logic merely intends to explicate common ways of inductive reasoning, the question of its usefulness leads back to the general question: Is it desirable that procedures which are generally applied, though only intuitively or instinctively, are brought into the clear daylight, analyzed and systematized in the form of exact rules? Whoever gives an affirmative answer to this general question will acknowledge the importance of the special problem of explicating inductive reasoning, that is, of constructing a system of inductive logic with rules as exact as those of the older, well-established system of deductive logic. Whether any particular system proposed as a solution for this problem is workable and fruitful is, of course, another question.

---

[1] Paper read at the Symposium on Philosophy of Science, joint session of the Am. Phys. Soc. and the Philosophy of Science Association, Chicago, 27 October 1951.

As I see it, the fundamental concept of inductive logic is *probability*. All inductive reasoning is probability reasoning. However, the word 'probability' is not unambiguous. I refer here to probability in one particular sense, the logical sense, which we might call *inductive probability*. This concept must be clearly distinguished from probability in the statistical sense. The distinction is practically important and theoretically fundamental.

Statistical probability is a certain quantitative physical characteristic of physical systems. Like any other physical magnitude it is to be established empirically, by observations. In this case the observations are of a statistical nature. They consist in counting frequencies. Statistical probability is obviously very closely connected with frequency, but it is not just the same as frequency. When we say that for a given die the probability of throwing an ace is 0.158, then this statement refers to a physical characteristic of the die and thus is not fundamentally different from statements about its mass, temperature, electric conductivity, etc. Imagine a fictitious physicist who, like the Laplacean superman, knows the present micro-state of the die in terms of the distribution of the particles and of the fields and, in addition, all the relevant laws. This physicist could, by purely mathematical calculations, find not only the present temperature of the die, its conductivity, etc., but also the probability of its yielding an ace if thrown under specified conditions. Since the micro-state is actually not known, the question arises how to test a statement about the probability, how to confirm or disconfirm it. The answer in the case of probability is not fundamentally different from that in the case of temperature or other physical magnitudes. The statement is to be tested by making experimental arrangements which lead to observable phenomena connected with the magnitude in question, whose value itself is not directly observable. To test the probability statement, we determine the relative frequency of aces in a sufficiently long series of throws of the die. This frequency is itself not the probability; it is rather a consequence of the probability state of the die, a consequence which is observable and therefore may serve for us as a symptom for the probability state, just as the expansion of the mercury column in the thermometer is not itself the temperature but an observable consequence of the temperature state and therefore a suitable means of testing a statement about the temperature. It is sometimes said that the statistical concept of probability involves a peculiar difficulty, since obviously no finite series of throws is sufficient to determine the probability with absolute precision and certainty. This is indeed true, but the same holds for all physical magnitudes. There is likewise no possible procedure for determining the temperature with absolute precision and certainty. The answer to the question: "How long then shall we make the series of throws with the die in order to determine the probability?" is the same as the answer to the question: "How fine a thermometer should we use to measure the temperature?" In both cases the answer depends, on the one hand, on the time and money available and, on the other hand, on the desired degree of precision. More specifically, it depends on the theoretical or practical advantages to be expected from higher precision. The finer the thermometer and the longer the series of throws, the higher the precision which is achieved. In neither case is there a perfect procedure. The concept of statistical probability may be introduced either by an explicit definition in terms of a limit as done by Mises and Reichenbach, or by an axiom system with rules of application as done by the majority of contemporary statisticians. In either case, the concept is logically legitimate and practically useful for work in statistics and in all branches of science which apply statistical methods. Thus I do not agree with those representatives of the inductive concept of probability, like Keynes and Jeffreys, who reject the statistical concept. On the other hand, I do not agree with Mises, Reichenbach, and the statisticians, who reject the inductive concept. Both concepts of probability are important for scientific work, each in its own field, the one within science itself, the other in inductive logic, which gives rules for certain operations with the statements of the language of science. The statistical concept is today generally recognized. Although certain problems connected with it are still under investigation, a defense of its legitimacy and usefulness is no longer necessary. The status of the inductive concept, however, is still debated. Therefore, today it is still necessary to defend its right of existence and to show its usefulness.

A statement of inductive probability states a relation between a hypothesis and a given body of evidence, e.g., results of actual or possible observations. The asserted probability value means the degree to which the hypothesis is confirmed or supported by the evidence. It is important to notice that a statement on inductive probability or *degree of confirmation* is relative to the evidence. This does not merely mean that the statement is based on or derived from observations. That is the case for *every* scientific statement. For example, the statement "The probability that it will rain tomorrow is 1/5" is incomplete unless we add "with respect to such and such an evidence", e.g., certain meteorological observations. For the validity of the statement it does not matter whether the evidence referred to in the statement is true and whether it is known to the speaker. To be sure, in the practical application of any inference, whether deductive or inductive, the premises are usually known. But that is not necessary. They may be unknown or they may even be known to be false. If this is so, what can be the basis of the validity of the probability statement itself, as distinguished from the validity of the hypothesis or the evidence? It can obviously not be of an empirical nature. All relevant empirical knowledge or assumption is contained in the evidence statement. In our example, we can empirically reexamine the truth of the evidence concerning past meteorological events. If we wait until tomorrow, we can empirically test the truth of the hypothesis that it will rain tomorrow. But in neither way can we test the truth of the probability statement itself. We shall see tomorrow either rain or not-rain, we may observe a rain of short or of long duration, a rain of high or low intensity, but we shall not see a rain of probability 1/5. Some critics of inductive logic have pointed to this fact and drawn from it the conclusion that, since the inductive probability statement is not empirically testable, it must be scientifically meaningless. Their mistake was that they regarded the probability statement as a factual synthetic statement. The statement is, however, of a purely logical nature. Hence there is no need and no possibility of empirical testing. A statement of inductive probability is in one respect similar to a statement in deductive logic: the relation between the hypothesis and the evidence which it asserts is a logical relation, similar to the deductive relations of deducibility or incompatibility, though weaker than those. If the statement asserts a probability value close to 1, then the probability relation hereby expressed is very close to the relation of deducibility: the hypothesis is nearly deducible from the evidence but not quite. On the other hand, if the stated probability value is near to 0, then the probability relation is close to the deductive relation of incompatibility: the hypothesis is nearly incompatible with the evidence but not quite. For any intermediate probability value the probability relation is more remote from the deductive relations which are, so to speak, the extreme cases. Thus inductive probability means in a sense partial deducibility. It is a logical relation inasmuch as it can be established, just as a deductive relation, as soon as the two statements of hypothesis and evidence are given, by merely applying logical analysis, in this case the rules of inductive logic, without the use of observations. Although the statement expresses only a logical relation, it has nevertheless significance. It draws boundaries to reasonable conduct. For example, if the probability of rain tomorrow is 1/5 with respect to the evidence available to an observer, then it would not be reasonable for him to bet on rain tomorrow at odds higher than 1:4.

If we recognize that statements on inductive probability have a purely logical character, then we are in a position to clear up a question which has been debated for two hundred years: the problem of the so-called principle of insufficient reason or *principle of indifference*. As I see it, the beginning of the development of inductive logic was made in the classical theory of probability by men like Bernoulli, Bayes, and Laplace. Many points of the classical theory, and among them also fundamental points, have been criticized for more than a hundred years and especially in our century. I think that this criticism is correct to a large extent. I agree with the critics that today it is impossible to go back to the classical conception. But I do not agree with those who say that the only way out is the total rejection of the classical conception. The classical principle of indifference states: "If no reasons are known which would favor one of several possible events, then the events are to be taken as equally probable.'' The usual objection against this principle is that it puts a premium on ignorance; if you do not know anything about the alternatives, then the principle allows you to make a certain statement about them; if you know certain things, then that statement is no longer permissible. To derive a statement from ignorance looks rather absurd. And it would indeed be absurd to apply this procedure to a factual statement. But the statement of equiprobability to which the principle of indifference leads is, like all statements on inductive probability, not a factual but a logical statement. If the knowledge of the observer does not favor any of the possible events, then with respect to this knowledge as evidence they *are* equally probable. The statement assigning equal probabilities in this case does not assert anything about the facts, but merely something about the logical relations between the given evidence and each of the hypotheses; namely, that these relations are logically alike. These relations are obviously alike if the evidence has a symmetrical structure with respect to the possible events. The statement of equiprobability asserts nothing more than the symmetry.

For these reasons I believe that the basic idea of the old principle of indifference is valid. On the other hand, there can be no doubt that many of the applications of this principle, especially in the earlier period, were invalid and that some of the conclusions drawn were even outright absurd. But I believe that the aim which the classical pioneers envisaged was valid. Our task is not to abandon entirely the classical conception, but to construct an exact concept of degree of confirmation explicating the classical conception of inductive probability on a more cautious and more solid foundation.

Now let us look at the function of inductive logic in the field of science. A scientist makes, on the one hand, observations of natural phenomena or of results of experiments. These constitute his evidence. On the other hand, he entertains hypotheses concerning facts not yet observed or even unobservable. The hypothesis may concern a single fact or it may be a conditional prediction of the form ''If we were to do such and such things, then such and such events would happen'', or it may have a general form, e.g., a statement about the value of a material constant, or a general law stating the relations between various physical magnitudes in terms of mathematical functions. The law may have a deterministic form or a statistical form, stating e.g., proportions, averages, or other statistical parameters of distributions of certain magnitudes. The purpose of the law is to explain known phenomena and predict new ones. The task of inductive logic is not to *find* a law for the explanation of given phenomena. This task cannot be solved by any mechanical procedure or by fixed rules; it is rather solved through the intuition, the inspiration, and the good luck of the scientist. The function of inductive logic begins *after* a hypothesis is offered for examination. Its task is to measure the support which the given evidence supplies for the tentatively assumed hypothesis. In particular, the task will often be to determine among several competing hypotheses the one which is most strongly confirmed by the given evidence. The competing hypotheses may, for example, concern the possible results of an experiment to be made, the possible causes of an observed event, or possible outcomes of a business investment. Or they may be various laws which are mutually incompatible, each of which might be regarded as an explanation of a given set of observational results concerning new phenomena not explained so far.

Sometimes an objection is raised against the idea of a system of inductive logic with exact rules for the determination of the degree of confirmation because of the fact that a scientist who chooses one among a number of considered hypotheses is influenced in this choice also by many non-rational factors and that he would not be willing to hand over the task of this choice to a machine or to have himself, so-to-speak, transformed into a machine which merely applies fixed rules. Now it is true that many non-rational factors affect the scientist's choice, and I believe that this will always be the case. The influence of some of these factors may be undesirable, for instance a bias in favor of a hypothesis previously maintained publicly or, in the case of a hypothesis in social science, a bias caused by moral or political preferences. But there are also non-rational factors whose effect is important and fruitful; for example, the influence of the "scientific instinct or hunch". Inductive logic does not intend to eliminate factors of *this* kind. Its function is merely to give to the scientist a clearer picture of the situation by demonstrating to what degree the various hypotheses considered are confirmed by the evidence. This logical picture supplied by inductive logic will (or should) influence the scientist, but it does not uniquely determine his decision of the choice of a hypothesis. He will be helped in this decision in the same way a tourist is helped by a good map. If he uses inductive logic, the decision still remains his; it will, however, be an enlightened decision rather than a more or less blind one.

In addition to judging the status of hypotheses, inductive logic has also the task of supplying rules of *estimation*. There is much discussion and controversy among statisticians concerning the validity of particular methods of estimation and the choice of a suitable method of estimation in a given problem situation. I believe that, if the basis of inductive logic is constructed by laying down rules for calculating the degree of confirmation, then it is possible to define a general estimate function in terms of degree of confirmation, applicable to all kinds of magnitudes expressible in the language in question. The definition which I propose takes as the estimate of the magnitude on the basis of a given body of evidence the weighted mean of the possible values of the magnitude, the weight of each value being its degree of confirmation with respect to the given evidence. This is the same as the expectation value of the magnitude (if we understand the term 'expectation value' in the inductive sense based on inductive probability, in contrast to its statistical sense based on statistical probability). To obtain a general method of estimation would be of great importance not only from a *theoretical* point of view but also for the problem of determining *practical* decisions in a rational way. Suppose a man has to make a decision in a given economic situation, e.g., concerning investments. This means that he has to choose one among a number of alternative actions possible to him in the situation. For each possible action he considers the various possible outcomes in terms of money gained. If he is able to determine the degree of confirmation for each possible outcome in the case of the considered action, he may calculate the sum of these gains, each multiplied with its degree of confirmation. This will be his estimate of the gain in the case of the action considered. In the same way, he may calculate the estimate of the gain for each of the possible actions. Then, if he is a rational man, he will choose that one among the possible actions for which the estimated gain has its greatest value. (A more exact procedure would consider, not the gain in terms of money, but the utility of this gain, i.e., the measure of satisfaction derived by the person from the gain.) Thus inductive logic serves as an instrument for the determination of rational decisions.

As mentioned earlier, the development of inductive logic began with the classical theory of probability. However, its systematization as a branch of modern logic is of recent origin, beginning with John Maynard Keynes thirty years ago. I have constructed a set of rules of inductive logic for a simple language system, which is restricted to qualitative descriptions of things without the use of measurable magnitudes (like temperature, electric current, etc.), but including statements of frequencies. These rules make possible the calculation of the degree of confirmation for any hypothesis and any body of evidence expressible in that language system and the calculation of the estimate of a frequency on the basis of any given evidence. The further development of inductive logic for more comprehensive language systems and finally for the language of science as a whole remains a task for the future.

A theory of inductive logic is systematically developed in my book *Logical Foundations of Probability,* Chicago, 1950. The underlying conception of inductive probability is explained in *The Nature and Application of Inductive Logic,* Chicago, 1951, which is a reprint of six non-technical sections from the book mentioned.

ON MENTAL ENTITIES

W. V. QUINE*

Harvard University

A question which is very much in the air this evening — very close to the discussion even when not directly discussed — is whether we should affirm or deny that there are such things as *sensations,* these being conceived as immediate, subjective experiences. I shall touch on this question, but not just yet. For a while it will be convenient to talk as if there are.

Falling in thus uncritically with the usage of old-fashioned epistemology and introspective psychology, let us consider, to begin with, the process of language. It has been the fashion in recent philosophy, both that of some of the English analysts and that of some of the logical positivists, to think of the terms of science and ordinary language as having some sort of hidden or implicit definitions which carry each such term back finally to terms relating to immediate experience. Now this view is clearly unrealistic. A better description, though countenancing the notion of immediate experience still, is as follows. On the one hand we have language, as an infinite totality of said or appropriately sayable phrases and sentences. On the other hand we have our sense experience, which, by a process of psychological association or conditioned response, is keyed in with the linguistic material at numerous and varied places. The linguistic material is an interlocked system which is tied here and there to experience; it is not a society of separately established terms and statements, each with its separate empirical definition. There is no separate meaning, in terms of direct experience, for the statement that there is a table here, or that there is a planet somewhere in outer space. The statement that there is the planet may be keyed with our sense experience by our seeing the planet, or by our merely noting perturbations in the orbits of other planets. And even the statement that there is a table right here may be keyed with our sense experience through touch *or* sight *or* hearsay. Again the statement that I have cut my finger may be tied with experience either by sight or by pain or both. I have often argued that it is mistaken even to try to distinguish between those scientific statements which are true by virtue of the meanings of our terms and those which are true or probable by inductive evidence. As Pierre Duhem urged, it is the system as a whole that is keyed to experience. It is taught by exploitation of its heterogeneous and sporadic links with experience, and it stands or falls, is retained or modified, according as it continues to serve us well or ill in the face of continuing experience.

---

* Paper presented at the Meeting of the Institute for the Unity of Science, General Seminar, held in Cambridge, Mass., November 18, 1952. This, the first of a series of meetings on the subject ''Science and Human Behavior'' under the chairmanship of B. F. Skinner, was devoted to the problem of private knowledge.

We get the system, in its main lines, from our forebears. As children learning the language, we get on to various simple terms and key phrases by direct association with appropriate experiences. When we have progressed a bit with this kind of learning, we learn further usages contextually. Eventually we are in a position to receive traditional doctrine a whole chapter at a time. Finally some men venture to revise the tradition here and there, for the sake of greater simplicity or better experiential links; and these are scientists.

So much for the individual's mastery of language and lore; but what of the origins of all this in the race? It would be irrational to suppose that those origins were rational. The prehistory of science was probably a composite of primitive unconscious symbolism of the Freudian kind, confusions of sign and object, word magic, wishful thinking, and a lazy acquiescence in forms whose motivation had been long forgotten. Biases in our conceptual schemes may have great utility in the systematizing of science, and therewith high survival value, despite humble origins in the random workings of unreason — just as chance mutations in the chromosome may launch a sturdy and efficient new race. Natural selection through the ages tends to favor the happy accidents at the expense of the unpropitious ones, in the evolution of ideas as in the evolution of living species.

As scientists we accept provisionally our heritage from the dim past, with intermediate revisions by our more recent forebears; and then we continue to warp and revise. As Neurath has said, we are in the position of a mariner who must rebuild his ship plank by plank while continuing to stay afloat on the open sea.

How do we decide on such retentions and revisions? To be more specific: how do we decide, apropos of the real world, what things there *are?* Ultimately, I think, by considerations of simplicity plus a pragmatic guess as to how the overall system will continue to work in connection with experience. We posit molecules, and eventually electrons, even though these are not given to direct experience, merely because they contribute to an overall system which is simpler as a whole than its known alternatives; the empirical relevance of the notion of molecules and electrons is indirect, and exists only by virtue of the links with experience which exist at *other* points of the system. Actually I expect that tables and sheep are, in the last analysis, on much the same footing as molecules and electrons. Even these have a continuing right to a place in our conceptual scheme only by virtue of their indirect contribution to the overall simplicity of our linguistic or conceptual organization of experience; for note that even tables and sheep are not direct sensations.

The notion of macroscopic objects, tables and sheep, differs from that of molecules and electrons mainly, from an epistemological point of view, in point of degree of antiquity. Molecules were posited consciously in historic times, whereas the positing of the external objects of common sense is an original trait of human nature. Men have believed in something very like our common-sense world of external objects as long, surely, as anything properly describable as language has existed; for the teaching of language and the use of it for communication depend on investing linguistic forms with intersubjectively fixed references. It would be senseless to speak of a motive for this archaic and unconscious posit, but we can significantly speak of its function and its survival value; and in these respects the hypothesis of common-sense external objects is quite like that of molecules and electrons.

Because the notion of external macroscopic objects is so fundamental both to the origins of language and to the continued learning of language, we may be pretty sure that it is here to stay, though electrons and other more hypothetical entities may, with the continued revisions of science, come and go. Experience is continually reminding us that it is over the external macroscopic objects that there is least semantical misunderstanding between speakers; it is naturally to tables and sheep and the like that we keep returning when there is trouble about new concepts.

Epistemologists, put off by the fact that macroscopic objects are epistemologically on the same footing as molecules and electrons, have looked to sense data — the raw content of sensation itself — as a more ultimate realm of entities. The ensuing difficulties are notorious. They may be seen most vividly if to begin with we think about memory. Our present data of our own past experiences are, on this theory, some sort of faint present replicas of past sense impressions; faint echoes of past sensation accompanying the blare of present sensation. Now it takes little soul-searching to persuade oneself that such double impressions, dim against bright, are rather the exception than the rule. Ordinarily we do not remember the trapezoidal sensory surface of a desk, as a color patch extending across the lower half of the visual field; what we remember is *that* there was a desk meeting such-and-such approximate specifications of form and size in three-dimensional space. Memory is just as much a product of the past positing of extra-sensory objects as it is a datum for the positing of past sense data.

What has been said just now of memory applies in some degree to the stream of sensory experience generally. It would be increasingly apparent from the findings of the Gestalt psychologists, if it were not quite apparent from everyday experience, that our selective awareness of present sensory surfaces is a function of present purposes and past conceptualizations. The contribution of reason cannot be viewed as limited merely to conceptualizing a presented pageant of experience and positing objects behind it; for this activity reacts, by selection and emphasis, on the qualitative make-up of the pageant itself in its succeeding portions. It is not an instructive over-simplification, but a basic falsification, to represent cognition as a discernment of regularities in an unadulterated stream of experience. Better to conceive of the stream itself as polluted, at each succeeding point of its course, by every prior cognition.

So the notion of pure sense datum is a pretty tenuous abstraction, a good deal more conjectural than the notion of an external object, a table or a sheep. It is significant that when we try to talk of the subjective we borrow our terminology from the objective: I feel as if I were falling, I have a sinking sensation, I feel on top of the world, I see pink elephants (better: I feel as if I were really seeing real pink elephants), etc. Even the terms which we have come to regard as strictly and immediately sensory, like 'red', are obviously objective in reference in the first instance: we learn the word 'red' by being confronted with an external object which our parent calls red, just as we learn the word 'sheep' by being confronted with an external object which our parent calls a sheep. When, at a certain stage of epistemological sophistication, we transfer the word 'red' to an alleged datum of immediate subjective sense experience, we are doing just what we do when we say we have a sinking sensation: I feel *as if* I were really, externally falling, and I feel *as if* I were really confronted by an external red object.

I suggest that it is a mistake to seek an immediately evident reality, somehow more immediately evident than the realm of external objects. Unbemused by philosophy, we would all go along with Dr. Johnson, whose toe was his touchstone of reality. Sheep are real, unicorns not. Clouds are real, the sky (as a solid canopy) not. Odd numbers are perhaps real, but prime even numbers other than 2 not. Everything, of course, is real; but there are sheep and there are no unicorns, there are clouds and there is (in the specified sense of the term) no sky, there are odd numbers and there are no even primes other than 2. Such is the ordinary usage of the word 'real', a separation of the sheep from the unicorns. Failing some aberrant definition which is certainly not before us, this is the only usage we have to go on.

The crucial insight of empiricism is that any evidence for science has its end points in the senses. This insight remains valid, but it is an insight which comes after physics, physiology, and psychology, not before. Epistemologists have wanted to posit a realm of sense data, situated somehow just me-ward of the physical stimulus, for fear of circularity: to view the physical stimulation rather than the sense datum as the end point of scientific evidence would be to make physical science rest for its evidence on physical science. But if with Neurath we accept this circularity, simply recognizing that the science of science is a science, then we dispose of the epistemological motive for assuming a realm of sense data. May we then make a clean sweep of mental entities?

I urged earlier that we decide what things there are, or what things to treat as there being, by considerations of simplicity of the overall system and its utility in connection with experience, so to speak. I say "so to speak" because I do not want to force the issue of recognizing experience as an entity or composite of entities. I have talked up to now as if there were such entities; I had to talk some language, and I uncritically talked this one. But the history of the mind-body problem bears witness to the awkwardness of the practice. We are virtually bound, as remarked earlier, to hold to an ontology of external objects; but it is moot indeed whether the positing of additional objects of a mental kind are a help or a hindrance to science. Or perhaps not so moot. At any rate it is moot or else it is clear that they are a hindrance.

To repudiate mental entities is not to deny that we sense or even that we are conscious; it is merely to report and try to describe these facts without assuming entities of a mental kind. What is spoken of in terms of the residual posited objects of science and common sense as my cut finger is keyed into our nervous responses in various ways; nerves from my eye and other eyes are involved, and nerves from my finger. Some persons are so situated as to be accessible to the stimuli which are most closely relevant to the phrase 'Quine's cut finger' and some are not. A dozen of us are in a position, this evening, for the appropriate stimulation of the eye, and one of us for the appropriate stimulation of the finger.

None of us is oriented to external objects quite like anyone else, for we occupy different positions, and while we exchange positions the objects age. None of us learned his words quite like anyone else. But we use them in sufficient systematic agreement for fair communication — which is no accident, since language is subject to the law of survival of the fittest. We manage to talk effectively about other people's cut fingers because of a pattern of habits connecting with present and past stimulation of the eye together with past stimulation, under optically similar circumstances, of our own fingers. The same is true of pain — but no argument against construing pain as a state of the physical organism. If we repudiate mental entities as entities, there ceases to be an iron curtain between the private and the public; there remains only a smoke screen, a matter of varying degrees of privacy of events in the physical world. Consciousness still retains a place, as a state of a physical object, if — following the suggestion made by Professor Deutsch in addressing this institute last year — we construe consciousness as a faculty of responding to one's own responses. The responses here are, or can be construed as, physical behavior. It is not the purpose of this view to leave any aspect of life out of account. The issue is merely whether, in an ideal last accounting of everything or a present practical accounting of everything we can, it is efficacious so to frame our conceptual scheme as to mark out a range of entities or units of a so-called mental kind in addition to the physical ones. My hypothesis, put forward in the spirit of a hypothesis of natural science, is that it is not efficacious.

A BIBLIOGRAPHY OF CYBERNETICS

F. Dermot Barrett and Herbert A. Shepard
Massachusetts Institute of Technology

INTRODUCTION

Norbert Wiener defines cybernetics as "the study of messages, and in particular of the effective messages of control," and as "the entire field of control and communication theory, whether in the machine or in the animal." His book, *Cybernetics*, is the central work to which other items in this bibliography are related.

Cybernetics owes many of its elements to the practical science of communication engineering. In recent years the demand for complex communication devices such as television, automatic control systems, and large-scale computing machines made the need for an adequate theory of communication evident. Wiener, Shannon, and many others have contributed to a general theory of communication and control which Wiener calls cybernetics. For communications engineers, therefore, there is nothing revolutionary in cybernetics as it applies to their own work. For biological and social scientists, however, cybernetics presents a new point of view and system of concepts for studying the organism and its relations with other organisms and with the inorganic environment.

The prime concept in cybernetics is information. The term information refers to the representation of an event. If information is transmitted without loss from a transmitter to a receiver, then the pattern which existed among the elements at the transmitter will be represented in the arrangement of other elements at the receiver. Patterns, not elements, are transmitted. Most physical sciences are primarily concerned with the transmission of matter and energy. Cybernetics is interested in matter and energy only insofar as they are bearers of information.

The central interest of cybernetics is in the way information is used for controlling processes. A control mechanism is a system which uses information from other systems for continuously determining its own action relative to them. Control systems include all those which can be called adjustive or adaptive: servomechanisms, organisms, and social organizations. Thus cybernetics provides conceptual apparatus applicable to an enormous range of phenomena.

In addition, cybernetics provides a new frame of reference for considering some long-standing philosophical problems. The construction of machines which reproduce a set of functions hitherto restricted to organisms has called for a re-examination of such matters as free will, teleology, consciousness and scientific method. Some of the most noteworthy explorations of the meta-scientific aspects and philosophical implications of cybernetics have been those undertaken by Karl W. Deutsch and by D. M. MacKay.

The bibliography has been selected with the interests of social scientists in mind and engineering applications have been omitted, except for general material on servomechanisms and communication systems. For convenience, the bibliography has been classified under six headings, although some works properly belong in more than one category. The first class, ''Popular Expositions,'' is intended for the casual or lay reader who seeks a non-technical treatment. The core literature is included under the heading ''Cybernetics.'' The third and fourth categories, ''Information Theory and Communication Theory'' and ''Servomechanisms,'' represent two major foundations or branches of the science. The fifth class, ''Cybernetics Concepts in the Biological and Social Sciences,'' consists of the literature concerned with applying the viewpoint of cybernetics to the life sciences. The rate of output of such literature is increasing rapidly, but the total quantity is still small enough that most of it could be included. It is likely, however, that within a short time some of the material included here will be seen as inferior in quality and of secondary importance. The sixth class, ''Relevant Works in Social Science, Biology, and Philosophy,'' represents a somewhat arbitrary selection. The very relevance of cybernetics to many scientific fields has made this necessary. Thus Cannon's *The Wisdom of the Body*, written nearly thirty years ago, is undoubtedly a ''relevant'' work since its viewpoint is in such close harmony with that of cybernetics, whereas Barnard's *Functions of the Executive* is a much less clear-cut case, and has been included for the sake of scattered insights rather than for its general approach.

I. POPULAR EXPOSITIONS

1. Berkeley, E. C. *Giant Brains.* New York: John Wiley & Sons, Inc., 1949.
2. Davis, Harry M. "Mathematical Machines." *Scientific American*, 180, No. 4 (April 1948), 28-40.
3. Eisenhart, Churchill. "Cybernetics: A New Discipline." *Science*, 109, No. 2834 (April 22, 1949), 397-399.
4. Gittleson, Yvette. "Sacred Cows in Collision." *American Scientist*, 38, No. 4 (Oct. 1950), 603-609.
5. Rabel, G. "Mathematical Instruments and Calculating Machines." *Science News*, VII. Harmondsworth, Middlesex, England: Penguin Books, Ltd., 112-125.
6. Roddam, T. "Communication Theory, Old and New." *Science News*, XII. Harmondsworth, Middlesex, England: Penguin Books, Ltd., July 1949, 105-121.
7. Rosenblith, Walter A. Book review of *Cybernetics* in *Annals of the American Academy*, 264 (July 1949), 187.
8. Shannon, C. E. "Chess-Playing Machine." *Scientific American*, 182, No. 2 (Feb. 1950), 48-52.
9. Von Neuman, John. Book review of *Cybernetics* in *Physics Today*, 2, No. 5 (May 1949), 33-34.
10. Walter, W. G. "An Imitation of Life." *Scientific American*, 182, No. 5 (May 1950).
11. Weaver, W. "The Mathematics of Communication." *Scientific American*, 181, No. 1 (July 1949), 11-15.
12. Wiener, N. "Cybernetics." *Scientific American*, 179, No. 5 (Nov. 1948), 14-19.
13. Wiener, N. *The Human Use of Human Beings: Cybernetics and Society.* Boston: Houghton Mifflin, 1950.
14. Wiener, N. "New Concept of Communication Engineering." *Electronics*, 22, No. 1 (Jan. 1949), 74-77.

II. CYBERNETICS

1. Berkeley, E. C. "The Relations Between Symbolic Logic and Large-Scale Calculating Machines." *Science*, 112, No. 2910 (Oct. 6, 1950), 395-399.
2. Brillouin, L. "Life, Thermodynamics, and Cybernetics." *American Scientist*, 37 (Oct. 1949), 554-568.
3. Brillouin, L. "Maxwell's Demon Cannot Operate: Information and Entropy" I. *Journal of Applied Physics*, 22, No. 3 (March 1951), 334-337.

4. Brillouin, L. "Physical Entropy and Information" II. *Journal of Applied Physics*, 22, No. 3 (March 1951), 338-343.
5. Brillouin, L. "Thermodynamics and Information Theory." *American Scientist*, 38 (Oct. 1950), 594-599.
6. Cherry, E. Colin. "A History of the Theory of Information." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 22-44.
7. Deutsch, K. "Higher Education and the Unity of Knowledge." *Goals for American Education.* Ninth Symposium of the Conference on Science, Philosophy, and Religion. New York: Harper & Bros., 1950, pp. 55-139. See especially Part VI, Sec. 2, pp. 102-136.
8. Deutsch, K. "Mechanism, Organism, and Society," Some Models in Natural and Social Science. *Philosophy of Science*, 18, No. 3 (July 1951), pp. 230-252.
9. Deutsch, K. "Mechanism, Teleology, and Mind: The Theory of Communications and Some Problems in Philosophy and Social Science." *Philosophy and Phenomenological Research*, XII, No. 2 (Dec. 1951), pp. 185-223.
10. Frank, L. K. Foreword to "Teleological Mechanisms." *Annals of the New York Academy of Sciences*, 50, Article 4 (Oct. 1948), 189-196.
11. Gabor, D. "Communication Theory and Physics." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 48-60.
12. Gabor, D. "La Théorie des Communications et La Physique." Editions de *La Revue D'Optique Théorique et Instrumentale*, Paris, 1951.
13. Kullback, S. and Leibler, R. A. "On Information and Sufficiency." *The Annals of Mathematical Statistics*, 22 (March 1951), 79-87.
14. MacKay, D. M. "Entropy, Time, and Information." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 162-166.

15. MacKay, D. M. "The Nomenclature of Information Theory." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 9-22.
16. MacKay, D. M. "Quantal Aspects of Scientific Information." *Phil. Magazine*, 7, 41 (1950), 293-311.
17. Mason, S. J. "On the Logic of Feedback." D. Sc. Thesis, M.I.T., 1951. Dittoed.
18. McCulloch, Warren S. "Finality and Form in Nervous Activity." Fifteenth James Arthur Lecture. American Museum of Natural History, New York, May 2, 1946. Multigraphed.
19. McCulloch, Warren S. and Pfeiffer, J. "Digital Computers Called Brains." *Scientific Monthly*, 69 (1949), 368-376.
20. McCulloch, Warren S. "A Recapitulation of the Theory, with a Forecast of Several Extensions," in "Teleological Mechanisms." *Annals of the New York Academy of Sciences*, 50, Article 4 (Oct. 1948), 247-258.
21. Oliver, B. M., Pierce, J. R., and Shannon, C. E. *The Philosophy of PCM.* Proceedings of the Institute of Radio Engineers (Nov. 1948).
22. Rafferty, J. A. "Mathematical Model in Biological Theory." *American Scientist*, 38, No. 4 (Oct. 1950), 549.
23. Rapoport, A. and Shimbel, A. "Mathematical Biophysics, Cybernetics and General Semantics." *ETC: A Review of General Semantics*, 6 (1949), 145-159.
24. Raymond, Richard C. "Communication, Entropy, and Life." *American Scientist*, 38, No. 2, 273-278.
25. Rosenblueth, A., Wiener, N., and Bigelow, J. "Behavior, Purpose, and Teleology." *Philosophy of Science*, X, No. 1 (Jan. 1943), 18-24.
26. Rosenblueth, A. and Wiener, N. "The Role of Models in Science." *Philosophy of Science*, XII (Oct. 1945), 316-322.
27. Shannon, C. E. "Programming a Computer for Playing Chess." *Philosophical Magazine*, Series 7, XLI (March 1950), 256-275.
28. Szilard, L. "Uber die Entropieverminderung in einem Thermodynamischen System bei Eingriffen intelligenter Wesen." *Zeitschr. f. Phys.* 53, 840-856. 1929.

29. Trimmer, John D. "Instrumentation and Cybernetics." *Scientific Monthly*, LXIX, No. 5 (Nov. 1949), 328-331.
30. Trimmer, John D. *Response of Physical Systems.* New York: Wiley and Sons, 1950.
31. Von Bertalanffy, L. "The Theory of Open Systems in Physics and Biology." *Science*, 111, No. 23 (1950).
32. Wiener, N. *Cybernetics.* Cambridge, Mass.: Technology Press, 1948.
33. Wiener, N. and Rosenblueth, A. "Purposeful and Non-Purposeful Behavior." *Philosophy of Science*, 17 (Oct. 1950).
34. Wiener, N. "Time, Communication, and the Nervous System" in "Teleological Mechanisms." *Annals of the New York Academy of Sciences*, 50, Article 4 (Oct. 1948), 197-219.

III. INFORMATION THEORY AND COMMUNICATION THEORY

1. Bartlett, M. S. "The Statistical Approach to the Analysis of Time-Series." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 81-102.
2. Brillouin, L. "Information Theory and Most Efficient Codings for Communication or Memory Devices." *Journal of Applied Physics*, 22, No. 9 (Sept. 1951), 1108-1111.
3. Clavier, A. G. "Evaluation of Transmission Efficiency According to Hartley's Expression of Information Content." *Electrical Communications* (Dec. 1948).
4. Cohen, F. *Some Analytical and Practical Aspects of Wiener's Theory of Prediction.* M.I.T. Research Laboratory of Electronics, Technical Report No. 69.
5. Earp, C. W. "Relationship between Rate of Transmission of Information, Frequency, Bandwidth and Signal-to-Noise Ratio." *Electrical Communications*, 25 (June 1948), 178-195.
6. Fano, R. M. *The Transmission of Information.* M.I.T. Research Laboratory of Electronics, Technical Report No. 65.
7. Fano, R. M. *The Transmission of Information,* Part II. M.I.T. Research Laboratory of Electronics, Technical Report No. 149.

8. Fisher, R. A. "The Logic of Inductive Inference." *Journal of the Royal Statistical Society*, Vol. XCVIII, Pt. 1 (1935), 39-54.
9. Gabor, D. "Communication Theory and Physics." *Philosophical Magazine* (Nov. 1950).
10. Gabor, D. "Theory of Communication." *Journal of the Institute of Electrical Engineering*, 93, Pt. 3 (Nov. 1946), 439.
11. Hamming, R. W. "Error Detecting and Error Correcting Codes." *Bell System Technical Journal*, 29 (April 1950), 147.
12. Hartley, R. V. L. "Transmission of Information." *Bell System Technical Journal* (July 1928), 535-537.
13. Laemmel, A. E. *General Theory of Communication*. Polytechnic Institute of Brooklyn. Microwave Research Institute. (introductory parts)
14. Laplume, J. "Sur le Nombre de Signaux Discernables . . ." *Comptes Rendus de l'Ac. des Sci.*, 226 (April 1948), 1348-1349.
15. Lee, Y. W. and Wiesner, J. B. "Correlation Functions and Communication Applications." *Electronics*, 23, No. 6 (June 1950), 86-92.
16. Rice, S. O. "Communications in the Presence of Noise: Probability of Error for Two Encoding Schemes." *Bell System Technical Journal* XXIX, No. 1 (Jan. 1950), 60-93.
17. Shannon, C. E. "Communication in the Presence of Noise." *Proceedings of the Institute of Radio Engineers*, 27 (Jan. 1949), 10-21.
18. Shannon, C. E. "Communication Theory-Exposition of Fundamentals." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 44-48.
19. Shannon, C. E. "Communications Theory of Secrecy Systems." *Bell System Technical Journal*, 28 (Oct. 1949), 656-715.
20. Shannon, C. E. "General Treatment of the Problem of Coding." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 102-105.

21. Shannon, C. E. "The Lattice Theory of Information." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 105-108.
22. Shannon, C. E. "A Mathematical Theory of Communication." *Bell System Technical Journal*, XXVII (July 1948), 379-424 and XXVII (Oct. 1948), 623-657.
23. Shannon, C. E. "Memory Requirements in a Telephone Exchange." *Bell System Technical Journal*, XXIX, No. 3 (July 1950), 343-349.
24. Shannon, C. E. and Weaver, W. *The Mathematical Theory of Communication*. Urbana, Ill.: Un. of Illinois Press, 1949.
25. Tuller, W. G. *Theoretical Limitations on the Rate of Transmission of Information*. M.I.T. Research Laboratory of Electronics, Technical Report No. 114.
26. Ville, J. A. "Théorie et Applications de Signal Analytique." *Cables et Transmission* (1948), 61.
27. Westcott, J. H. "Criteria of Prediction and Discrimination." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 153-162.
28. Wiener, N. *Extrapolation, Interpolation and Smoothing of Stationary Time Series*. New York: Wiley and Sons, 1949.
29. Woodward, P. M. "Theory of Radar Information." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 108-114.

IV. SERVOMECHANISMS

1. Ahrendt, W. R. and Taplin, J. F. *Automatic Regulation—A Discussion of Fundamentals*. Preliminary ed. Washington: U. S. Library of Congress, 1947.
2. Black, H. S. "Stabilized Feedback Amplifiers." *Electrical Engineering*, Vol. 53 (1934), 114.
3. Brown, G. S. and Campbell, D. P. *Principles of Servomechanism: Dynamics and Synthesis of Closed Loop Control Systems*. New York: Wiley and Sons, 1948.

4. James, H. F., Nichols, N. B. and Philips, R. S. *Theory of Servomechanisms.* New York: Penguin Books, Inc., 1948.
5. Lauer, H., Nesnick, R. and Matson, L. E. *Servomechanism Fundamentals.* New York: McGraw-Hill, 1945.
6. MacColl, L. A. *Servomechanisms.* New York: Van Nostrand, 1945.

V. CYBERNETICS CONCEPTS IN THE BIOLOGICAL AND SOCIAL SCIENCES

1. "Application of Operational Analysis to Human Motor Behavior." *Aero Medical Lab. Memo Report,* MCREXD 694-2J (May 3, 1948).
2. Ashby, W. R. "The Behavioral Properties of Systems in Equilibrium." *American Journal of Psychology,* 59 (1946), 682-686.
3. Ashby, W. R. "Effects of Control on Stability." *Nature,* 155 (1945), 242-243.
4. Barrett, Eric. "Cybernetics as Applied to a Study of Normal and Abnormal Adjustment Mechanisms." *The Journal of Psychology,* 30, first half (July 1950), 11-31.
5. Barrett, E. and Post, G. "Introduction to Some Principles of Applied Cybernetics." *The Journal of Psychology,* 30, first half (July 1950), 3-10.
6. Bates, J. A. V. "Significance of Information Theory to Neurophysiology." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 137-143.
7. Bateson, G. "Bali—The Value System of a Steady State." *Social Structure: Studies Presented to A. R. Radcliffe-Brown.* Ed. by Meyer Fortes. Oxford: Clarendon Press, 1949.
8. Bavelas, Alex. "Communication Patterns in Task-Oriented Groups." *The Journal of the Acoustical Society of America,* 22, No. 6 (Nov. 1950), 725-730.
9. Bavelas, Alex and Barrett, Dermot. "An Experimental Approach to Organizational Communication." *Personnel* (April 1951).
10. Bavelas, Alex et al. "Human Communications Systems." *Quarterly Progress Report,* Research Laboratory of Electronics, M.I.T. (July 15, 1950), 81-86.

11. Churchman, C. W. and Ackoff, R. L. "Purposive Behavior and Cybernetics." *Social Forces,* 29, No. 1 (Oct. 1950), 32-39.
12. Cobwin, H. E. "The Brain Analogy." *Psychological Review,* 58 (1951), 155-178.
13. Craik, K. J. W. "Theory of the Human Operator in Control Systems:" I. "The Operator as an Engineering System." *British Journal of Psychology,* No. 38 (1947), 56-61.
14. Craik, K. J. W. "Theory of the Human Operator in Control Systems:" II. "Man as an Element in a Control System." *British Journal of Psychology,* No. 38 (1948), 142-148.
15. Deutsch, K. W. "Innovation, Entrepreneurship, and the Learning Process." *Change and the Entrepreneur.* Cambridge: Harvard Un. Press, 1949.
16. Deutsch, K. W. "Nationalism, Communication and Community." *Perspectives on a Troubled Decade.* 10th Conference Symposium on Science, Philosophy, and Religion. New York: Harper & Bros., 1950.
17. Deutsch, K. W. *Nationalism and Social Communication.* To be published by the Technology Press in 1953.
18. Deutsch, K. W. "Nationalism and the Social Scientists." *Foundations of World Organization.* 11th Conference Symposium on Science, Philosophy, and Religion. Ed. by Lyman Bryson et al. New York: Harper & Bros. To be published in 1953.
19. Deutsch, K. W. "Some Notes on Research on the History of Entrepreneurship, Innovation, and Decision-Making." *Explorations in Entrepreneurial Research* (May 1949), 8-16.
20. Dunbar, M. J. "Feedback Systems and Oceanography." *American Scientist,* 38, No. 4 (Oct. 1950), 599-603.
21. Ellson, D. G. "The Application of Operational Analysis to Human Motor Behavior." *Psychological Review,* No. 56 (1949), 9-17.
22. Fano, R. M. "The Information Theory Point of View in Speech Communication." *Journal of the Acoustical Society of America,* 22, No. 6 (Nov. 1950), 691-696.
23. Frick, F. C. and Klemmer, E. T. "Assimilation of Information from Dot and Matrix Patterns." *The American Psychologist,* 5, No. 8 (Aug. 1951), 267.

24. Fry, D. B. "Communication Theory and Linguistic Theory." *Report of Proceedings. Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950, Mimeographed, pp. 120-125.
25. Gabor, D. "Acoustical Quanta and the Theory of Hearing." *Nature*, 159, No. 303 (1947).
26. Gabor, D. "New Possibilities in Speech Transmission." *Journal of the Institute of Electrical Engineering*, Part III (Nov. 1947) and Vol. 95, Part III (Sept. 1948).
27. Gerard, Ralph W. "Some of the Problems Concerning Digital Notions in the Central Nervous System" in *Cybernetics—Circular Causal and Feedback Mechanisms in Biological and Social Systems.* Transactions of the Seventh Conference on Cybernetics, March 23-24, 1950, New York. Ed. by Heinz von Foerster. New York: Josiah Macy, Jr. Foundation, 1951, pp. 11-57.
28. Goodwin, R. M. "Dynamic Coupling with Especial Reference to Markets Having Production Lags." *Econometrica,* 15, p. 181.
29. Hertz, D. and Livingston, R. T. "Contemporary Organizational Theory." *Human Relations*, Winter 1950, III, 4.
30. Hick, W. E. "Information Theory in Psychology." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 130-134.
31. Homans, G. *The Human Group.* New York: Harcourt Brace, 1950.
32. Huggins, William H. "Linear Systems and Group Dynamics." 1949. Mimeographed paper.
33. Huggins, William H. and Licklider, J. C. R. "Place Mechanisms of Auditory Frequency Analysis." *Journal of the Acoustical Society of America,* 23, No. 3 (May 1951), 290-299.
34. Hutchinson, G. E. "Circular Causal Systems in Ecology" in "Teleological Mechanisms." *Annals of the New York Academy of Sciences*, 50, Article 4 (Oct. 1948), 221-245.
35. Kreezer, G. L. "An Inverse Decibel Log Frequency Method for Determination of the Transfer Functions

of Psychological Systems." *Science* (Dec. 23, 1949), 681-684.

36. Kubie, Lawrence S. "Neurotic Potential and Human Adaptation." *Conference on Cybernetics.* (Transactions of the Sixth Conference, March 24-25, 1949, New York). New York: Josiah Macy, Jr., Foundation, 1950, pp. 64-112.
37. Kubie, Lawrence S. "The Relationship of Symbolic Function in Language Formation and in Neuroses" in *Cybernetics—Circular Causal and Feedback Mechanisms in Biological and Social Systems.* Transactions of the Seventh Conference on Cybernetics, March 23-24, 1950, New York. Ed. by Heinz von Foerster. New York: Josiah Macy, Jr., Foundation, 1951, pp. 209-235.
38. Leavitt, H. J. "Some Effects of Certain Communication Patterns on Group Performance." *Journal of Abnormal and Social Psychology*, 46, No. 1 (Jan. 1951), 38-50.
39. Lee, Bernard S. "Effects of Delayed Speech Feedback." *Journal of the Acoustical Society of America*, 22, No. 6 (Nov. 1950), 824-826.
40. Lewin, Kurt. "Frontiers in Group Dynamics, II." *Human Relations*, 1, No. 2, 1947.
41. Licklider, J. C. R. "The Intelligibility and Information Content of Quantized Speech." *American Psychologist*, 4 (1949), 234.
42. Licklider, J. C. R. "The Manner in Which and Extent to Which Speech Can Be Distorted and Remain Intelligible" in *Cybernetics—Circular Causal and Feedback Mechanisms in Biological and Social Systems.* Transactions of the Seventh Conference on Cybernetics, March 23-24, 1950, New York. Ed. by Heinz von Foerster. New York: Josiah Macy, Jr., Foundation, 1951, pp. 58-122.
43. Licklider, J. C. R. and Miller, G. A. "The Perception of Speech" in *Handbook of Experimental Psychology.* Ed. by S. S. Stevens. New York: John Wiley and Sons, 1951, pp. 1040-1075.
44. Luce, R. D. "Connectivity and Generalized Cliques in Sociometric Group Structures." *Psychometrika,* 15 (1950), 169-190.

45. Luce, R. D. and Perry, A. D. "A Method of Matrix Analysis of Group Structure." *Psychometrika,* 14 (1949), 95-116.

46. Mayne, R. "Some Engineering Aspects of the Mechanism of Body Control." *Electrical Engineering,* 70, 3 (1951).

47. McCulloch, W. C. and Pitts, W. "A Logical Calculus of the Ideas Immanent in Nervous Activity." *Bulletin of Mathematical Biophysics,* V (1943), 115-133.

48. Mead, Margaret. "Experience in Learning Primitive Languages Through the Use of Learning High Level Linguistic Abstractions" in *Cybernetics—Circular Causal and Feedback Mechanisms in Biological and Social Systems.* Ed. by Heinz von Foerster. Transactions of the Seventh Conference on Cybernetics, March 23-24, 1950, New York. New York: Josiah Macy, Jr., Foundation, 1951, pp. 159-185.

49. Miller, G. A. *Language and Communication.* New York: McGraw-Hill, 1951.

50. Miller, G. A. "Language Engineering." *Journal of the Acoustical Society of America,* 22, No. 6 (Nov. 1950), 720-725.

51. Miller, G. A. "Population, Distance and the Circulation of Information." *American Journal of Psychology,* 60 (1947), 276-284.

52. Miller, G. A. "Speech and Language" in *Handbook of Experimental Psychology.* Ed. by S. S. Stevens. New York: John Wiley and Sons, 1951, pp. 789-810.

53. Miller, G. A. and Frick, Frederick C. "Statistical Behavioristics and Sequences of Responses." *Psychological Review,* 56, No. 6 (Nov. 1949).

54. Miller, G. A., Heise, G. A., and Lichten, W. "The Intelligibility of Speech as a Function of the Context of Test Materials." *Journal of Experimental Psychology,* 41, No. 5 (May 1951).

55. Miller, G. A. and Selfridge, J. A. "Verbal Context and the Recall of Meaningful Material." *The American Journal of Psychology,* LXIII (April 1950), 176-185.

56. Morehouse, et al. "An Electroc-Analog Method of Investigating Problems in Economic Dynamics: Inventory Oscillations." *Econometrica,* 18 (Oct. 1950), 313-328.

57. Northrop, F. S. C. "The Neurological and Behavioristic Basis of the Ordering of Society by Means of Ideas." *Science*, 107, No. 2782 (April 23, 1948).

58. Ostow, M. "Entropy Changes in Mental Activity." *Journal of Nervous and Mental Disease*, 110 (Dec. 1949), 502-506.

59. Ostow, M. "The Entropy Concept and Psychic Function." *American Scientist*, 39 (Jan. 1951), 140-145.

60. Pollack, Irwin. "The Assimilation of Sequentially Encoded Information." *The American Psychologist*, 5, No. 8 (Aug. 1951), 266-267.

61. Raymond, R. C. "The Well-Informed Heat Engine." *American Journal of Physics*, 1951.

62. Richards, Paul I. "Machines Which Can Learn." *American Scientist*, 39, No. 4 (Oct. 1951), 711-716.

63. Ruesch, J. and Bateson, G. *Communication*. New York: W. W. Norton, 1951.

64. Rushton, W. A. H. "The Problem of the Information Which the Brain Receives from the Eye." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 128-130.

65. Shannon, C. E. "Prediction and Entropy of Printed English." *Bell System Technical Journal*, XXX, No. 1 (Jan. 1951), 50-64.

66. Shannon, C. E. "The Redundancy of English" in *Cybernetics—Circular Causal and Feedback Mechanisms in Biological and Social Systems*. Transactions of the Seventh Conference on Cybernetics, March 23-24, 1950, New York. Ed. by Heinz von Foerster. New York: Josiah Macy, Jr. Foundation, 1951, pp. 123-158.

67. Shepard, H. A. "Social System of a Research Group." Mimeographed.

68. Shimbel, Alfonso. "Input-Output Problems in Simple-Nerve Ganglion Systems." *Bulletin of Mathematical Biophysics*, 11 (1949), 165-171.

69. Simon, H. A. "An Exploration into the Use of Servomechanism Theory in the Study of Production Control." Cowles Commission Discussion Paper: *Economics*, No. 288. Mimeographed copy for private circulation.

70. Stagner, R. "Homeostasis as a Unifying Concept in

Personality Theory." *Psychological Review* 58 (1951), 5-17.

71. Straus, O. H. "The Relation of Phonetics and Linguistics to Communication Theory." *Journal of the Acoustical Society of America,* 22, No. 6 (Nov. 1950), 709-711.
72. Stroud, John. "The Development of Language in Early Childhood" in *Cybernetics—Circular Causal and Feedback Mechanisms in Biological and Social Systems.* Transactions of the Seventh Conference on Cybernetics, March 23-24, 1950, New York. Ed. by Heinz von Foerster. New York: Josiah Macy, Jr., Foundation, 1951, pp. 205-208.
73. Stroud, John. "Psychological Moment in Perception." *Conference on Cybernetics.* (Transactions of the Sixth Conference, March 24-25, 1949, New York.) New York: Josiah Macy, Jr., Foundation, 1950, pp. 27-64.
74. Tilts, P. M., ed. *Human Engineering for an Effective Air-Navigation and Traffic Control System.* No. 12, p. 88. National Research Council, March 1951.
75. Uttley, A. "Information, Machines and Brains." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 143-150.
76. von Foerster, Heinz. "Quantum Theory of Memory." *Conference on Cybernetics.* (Transactions of the Sixth Conference, March 24-25, 1949, New York.) New York: Josiah Macy, Jr., Foundation, 1950, pp. 112-134.
77. Walter, W. Grey. "The Functions of Electrical Rhythms in the Brain." *Journal of Mental Science,* XCVI, No. 402 (Jan. 1950).
78. Walter, W. Grey. "A Machine That Learns." *Scientific American,* 185, No. 2 (Aug. 1951), 60-63.
79. Walter, W. Grey. "Possible Features of Brain Function and Their Imitation." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 134-137.
80. Weinberg, M. "Mechanism in Neurosis." *American Scientist,* 39 (Jan. 1951), 74-99.
81. Werner, Heinz. "On the Development of Word Meanings" in *Cybernetics—Circular Causal and Feedback Mechanisms in Biological and Social Systems.* Transactions of the Seventh Conference on Cybernetics,

March 23-24, 1950, New York. Ed. by Heinz von Foerster. New York: Josiah Macy, Jr., Foundation, 1951, pp. 187-204.

82. Wiener, N. "Sensory Prothesises." *Conference on Cybernetics.* (Transactions of the Sixth Conference, March 24-25, 1949, New York.) New York: Josiah Macy, Jr., Foundation, 1950, pp. 203-208.
83. Wiener, N. "Speech, Language, and Learning." *Journal of the Acoustical Society of America,* 22, No. 6 (Nov. 1950), 696-697.

VI. RELEVANT WORKS IN SOCIAL SCIENCE, BIOLOGY, AND PHILOSOPHY

1. Adolph, A., ed. *Physiological Regulations.* Lancaster: Cartell Press, 1943.
2. Babbage, H. P. *Babbage's Calculating Engines.* London: Spon. Ltd., 1889.
3. Barnard, Chester I. *Functions of the Executive.* Cambridge: Harvard Un. Press, 1948. (See index for references to organization communications.)
4. Bartlett, M. S. "Probability in Logic, Mathematics and Science." *Dialectica,* III (1949), 104-112.
5. Bodmer, Frederick. *The Loom of Language.* London: G. Allen and Unwin, Ltd., 1943.
6. Cannon, W. *The Wisdom of the Body.* New York: W. W. Norton Co., 1923.
7. Chapple, E. D. "Measuring Human Relations: An Introduction to the Study of the Interaction of Individuals." *Genet. Psychol. Monogr.,* 22 (1940), 1-147.
8. Churchman, C. W. and Ackoff, R. L. "An Experimental Definition of Personality." *Philosophy of Science,* 14 (1947), 304-332.
9. Coyle, Grace L. *The Social Process in Organized Groups.* New York: R. R. Smith, Inc., 1930. Especially Chs. 4, 5, 6.
10. Eliot, T. D. "Reactions to Predictive Assumptions." *American Sociological Review,* 2 (1937), 508-517.
11. Festinger, L. et al. *Theory and Experiments in Social Communication.* Research Center for Group Dynamics, Ann Arbor, Michigan.
12. Gibson, J. J. *The Perception of the Visual World.* Boston: Houghton-Mifflin, 1950.

13. Goldstein, K. *Human Nature.* Cambridge: Harvard Un. Press, 1940. Chs. IV and V.
14. Hartree, D. R. *Calculating Instruments and Machines.* Urbana, Ill.: Un. of Illinois Press, 1949.
15. Hebb, D. C. *The Organization of Behavior.* New York: Wiley & Sons, 1949.
16. Hertz, D. and Lesser, S. "People in Groups." *Scientific American,* 184, No. 2 (Feb. 1951), 26-28.
17. Householder, A. S. and Landahl, H. D. *Mathematical Biophysics of the Central Nervous System.* Bloomington, Ind.: Principia Press, 1945.
18. Innis, H. A. "The Bias of Communications." *Canadian Journal of Economic and Political Science,* XV, No. 4 (Nov. 1949), 457-476.
19. Korzybski, A. *Science and Sanity.* 3rd ed. Lancaster, Pa.: Science Press, 1948.
20. Langer, S. K. *Philosophy in a New Key.* Cambridge: Harvard Un. Press, 1942. New York: Penguin Books, Inc., 1948.
21. Licklider, J. C. R. "A Duplex Theory of Pitch Perception." *Experientia,* VII, No. 4 (1951), 128-134.
22. Lotka, A. J. *Elements of Physical Biology.* Baltimore: Williams and Wilkins, 1925.
23. Moore and Tumin. "Some Social Functions of Ignorance." *American Sociological Review,* 14, No. 6 (Dec. 1949), 787-795.
24. Morris, C. W. *Signs, Language and Behavior.* New York: Prentice-Hall, 1946.
25. Newman, E. B. "Computational Methods Useful in Analyzing Series of Binary Data." *The American Journal of Psychology,* LXIV, No. 2 (April 1951), 252-262.
26. Newman, E. B. "The Pattern of Vowels and Consonants in Various Languages." *American Journal of Psychology,* 64 (1951), 369-379.
27. Ogden, C. K. and Richards, I. A. *The Meaning of Meaning.* New York: Harcourt Brace & Co.
28. Pigors, P. *Effective Communication in Industry.* National Association of Manufacturers, Lt. Rush Toland Memorial Study, No. 1.
29. Rapoport, A. *Science and The Goals of Man.* New York: Harper & Bros., 1950.

30. Rashevsky, N. *Mathematical Biophysics.* Chicago: University of Chicago Press, 1948.

31. Rashevsky, N. "Mathematical Biophysics of Abstraction and Logical Thinking." *Bulletin of Mathematical Biophysics,* 7, pp. 133-148.

32. Rashevsky, N. "Some Remarks on the Boolean Algebra of Nervous Nets in Mathematical Biophysics." *Bulletin of Mathematical Biophysics,* 7, 204-211.

33. Rashevsky, N. "The Neural Mechanism of Logical Thinking." *Bulletin of Mathematical Biophysics,* 8, No. 1 (Mar. 1946), 29-40.

34. Richardson, L. F. "Generalized Foreign Politics." *British Journal of Psychology.* Monograph Supplement XXIII, 1939.

35. Ruesch, J. and Bateson, G. "Structure and Process in Social Relations." *Psychiatry,* 12, No. 2 (May 1949), 105-124.

36. Samuelson, P. A. *Foundations of Economic Analysis.* Cambridge: Harvard Un. Press, 1947. Especially Part II and Appendix B.

37. Sapir, Edward. "Communication." *Encyclopedia of the Social Sciences.*

38. Schneirla, T. D. "The Levels Concept in the Study of Social Organization in Animals." *Social Psychology at the Crossroads.* Ed. by Rohrer and Sherif. New York: Harper & Bros., 83-120.

39. Schneirla, T. D. and Piel, G. "The Army Ant." *Scientific American,* 178, No. 6 (June 1948), 22-26.

40. Schrodinger, E. *What Is Life?* New York: The Macmillan Co., 1945. See especially Ch. 6.

41. Simon, H. A. *A Study of Decision-Making Processes in Administrative Organization.* New York: Macmillan Co., 1947. Chs. 2, 3, 10, 11.

42. Simon, H. A. *Administrative Behavior.* New York: The Macmillan Co., 1948.

43. Simon, H. A., Smithburg, D. W., Thompson, V. A. *Public Administration.* New York: Alfred A. Knopf, 1950.

44. Slater, Eliot. "Statistics for the Chess Computer and the Factor of Mobility." *Report of Proceedings, Symposium of Information Theory* (held in London, England, September 1950). London: Ministry of Supply, 1950. Mimeographed, pp. 150-153.

45. Tolman, E. C. *Purposive Behavior in Animals and Men.* Berkeley: Un. of California Press, 1932.
46. Weaver, Warren. "Science and Complexity." *American Scientist*, 36, No. 4, 536-544.
47. Whorf, B. L. *Four Articles on Metalinguistics.* Reprinted from *Technology Review* and *Language, Culture, and Personality.* Washington, D. C.: Foreign Service Institute, Dept. of State, 1950.
48. Zipf, G. K. *Human Behavior and the Principle of Least Effort.* Cambridge: Addison Wesley, 1949.

Acknowledgements

The authors wish to acknowledge their indebtedness to a number of friends who have brought to their attention many of the items included in this bibliography. Inclusions have been suggested by Professors K. W. Deutsch, R. L. Fano, and W. A. Rosenblith of M.I.T.; by Professor H. A. Simon of the Carnegie Institute of Technology; and by various members of a graduate seminar in social psychology given at M.I.T.

SIGNIFICS
ITS TENDENCY, METHODOLOGY, AND APPLICATIONS

D. Vuysje

PAGE

I Tendency 224
II Methodology 228
III Applications 235
IV Concluding Remarks 251
V Historical Development 254
Glossary 261
Bibliography 263

Preface

This monograph aims to give a survey of the development of signific studies during these last thirty-five years and to show the place the signific movement occupies among movements having similar tendencies, like Logical Empiricism, Operationism, Pragmatism and Semantics.

To *Professor Philipp Frank,* who has been an active member of the *International Society for Significs* for some years, I owe the suggestion to write a monograph in which the signific principles should be described and which should stress the similarity and the difference between significs and other theories of signs. I had the good fortune to discuss with him personally some of the fundamental problems occurring in this treatise, and to him go my warm thanks for his encouragement and help.

It may be obvious that the conceptions gathered in a monograph of this kind represent a selection of the contributions of many writers and that, also with a view to its conciseness, it is not possible to trace individual discoveries back to their authors. Moreover, in many cases it is extremely difficult to trace the personal influences which have given rise to certain views. Considering this, I have followed the procedure to add the names of the writers only to those statements which, according to my subjective judgment, are characteristic of the personal conceptions of the authors and have fruitfully influenced research in a signific direction. I may be excused if I do not name the many other scientists who have given valuable contributions to signific investigations.

One exception should be made in this respect with regard to my old friend *Professor G. Mannoury.* The discussions on significs I have had with him regularly for nearly twenty years, on problems referring to the relation between linguistic phenomena and human behavior, have more than anything else influenced my conceptions, which have partly found expression in this monograph, in which I have tried to shape significs in its essential lines. I am particularly indebted to him for his fine and stimulating spirit of cooperation which I have had the privilege of enjoying during these years and, in this special case, I am much obliged to him for the suggestions he made after reading the manuscript.

My thanks also go to my friend *Dr. H. J. Groenewold,* who has also been willing to read the manuscript and whose critical remarks have been of great advantage to me.

And, finally, I wish to express my deep gratitude to my friends and co-members of the *International Society for Significs;* without my personal participation in the activities of this circle I would not have been able to write this monograph, which, I hope, may be a modest contribution to the theory of signs and its applications.

D. VUYSJE

Amsterdam, June 1952

I. Tendency

One could say that from the outset signific studies have had a threefold aspect: a linguistic one, a psychological one and an epistemological one. On the one hand, the construction of efficient terminologies has been up to the present time one of the items of signific programs; on the other hand, the signific approach has implied a psychological investigation of the existing means of understanding, a psychological analysis of the linguistic behavior of individuals and groups. And more often than not this signific activity has been associated with an inquiry into the foundations of epistemological problems.

The program drawn up in 1919 by the *International Institute for Philosophy* at Amsterdam — one of the precursors of the *International Society for Significs* — already gave expression to this triple aspect and, although it was not carried out at that time, it nevertheless contained valuable directives for later signific groups. Taking as a starting-point the almost universally admitted opinion that language is not able to represent or to render any part of ''reality'' adequately, but that the ''meaning'' of words is merely dependent on the effect the speaker aims at or the hearer undergoes, the authors wished to point out that this changeability and relativity of significance, inherent in language, can reveal themselves in a different degree and in different forms. In general, it may be said that, according as the needs of man require more differentiated actions, language, too, should be more differentiated and more stabilized to that purpose. This stability was furthered externally by writing and printing, internally by what might be called the organization of language, i.e., by establishing the connection of the significance of a word with that of other words. This becomes evident in *definitions*, such as occur in the most exact form in mathematics and juridical science, but which are present in any discipline. To these phenomena of organization also belong all connections of words, such as have been passed on by custom and usage, without being fixed explicitly by the authority of government and science. These connections of words already play an important part in the early linguistic behavior of the child, and to them we may attribute the slight occurrence of ''basic words'' in children's talk, i.e., of words the child is not acquainted with by means of other words, but only by indication and imitation. Soon, the syntactic connections used by the adults (causal, oppositional, temporal, and others) largely influence the word-images of the child, and this influence tends to increase the stability of its small stock of words. After a few years a conversational language of a seemingly stable form has developed to such an extent that the phenomena which have been described as ''changeability'' and ''relativity'' of significance, so to say, may be rediscovered by reflection and observation of language. In the history of language (in the field of linguistics, too, phylogenesis and ontogenesis go hand in hand) a constant, but not regular, increase of the stability of words can be observed: a development from the poetical languages of the remote ages to the more positive mode of expression of the Greeks and Romans, the artful copiousness of the mediaeval philosophers, and, finally, the complex systems of language of modern natural and juridical science.

A new vocabulary was devised, based on a distinction between different levels of language, on the understanding that, with the exception of the words occurring in the explanatory and describing text, the words of each level imply only acquaintance with those of the preceding levels. The consideration underlying this division was that either the syntactic connections on some levels are not or are hardly discernible (primitive language and children's talk), or that they are easily discernible (expressive language), or that they have become the essential element of communication (colloquial language), and that on other, ''higher'' levels, especially the scientific and the symbolic ones, these connections are founded, at least for the greater part, on conventions or on postulated formulas regarding the symbols used.

It was expected that by this procedure the relativity of the significance of words would make itself apparent, and that those ''human needs'' of which the wording till then had been excluded from the scope of the prevailing word connections would come to be carefully considered; furthermore, that the discrimination between the emotive and the indicative value of words would prove to be a great help for unravelling inveterate misunderstandings existing in almost every domain of human mental activity.

These tendencies revealed themselves also in the ''declaration of principles'' of the Signific Circle (1922-1924)[1] which, besides, emphasized the empirical aspect of the signific approach.

''The significance of an act of communication for speaker and hearer cannot but partly be judged from the words and symbols used and can only approximately be expressed in other words. However, there is a great difference in the measure in which this analysis of words and this approximation are possible. In the language of science (especially in mathematical language) and, in a small measure, in that of technology, a rather great stability in the signification of words and linguistic acts can be attained by indication and gradual defining, while primitive, passionate and poetical language, and even colloquial language, is not appropriate to the introduction of definitions (in the proper sense of the word). On passionate and poetical levels of language there are very complicated complexes of conscious and half-conscious elements of thought and feeling, inherent in the word-images, although — it goes without saying — neither the intellectual aspects nor the emotive and volitive ones are entirely absent in any linguistic act.

"From this distinction result various shades of linguistic trends, of which the logico-formalistic and the immediate-intuitive ones may be considered as extreme forms and which constitute, together and in mutual connection, the contents of a signific linguistic investigation.

"This investigation should be carried on more systematically than before and include, besides an introspective investigation into the subconscious elements, the application of experimental and statistic methods."

A circular published in 1923 by the same circle and requesting cooperation for a revision of the nomenclature, especially in the peripheral domains, of the following branches of science: *Linguistics, psychology, theology, logic, sociology, jurisprudence, ethnology, mathematics, physics, chemistry* and *biology,* proved to be attractive to a number of scientists in the Netherlands and in other countries who were dissatisfied with the use of slogans and non-analyzed terms even in the field of the "exact sciences."

The fundamental principles outlined above also found expression in the program of the group of scientists who united round the journal *Synthèse* in 1937 and who laid the foundations for the activities of the *International Society for Significs*, whose main objective has been described as: "The practising, on an international scale, of analytic and synthetic significs in general, and its application to the theories of the foundations of the sociological, cultural, political and exact sciences in particular."

The distinction referred to in this item provides for research from an analytic and a synthetic point of view. The analytic approach deals with a signific investigation of a given language, and more particularly with an inquiry into the psychological background of the existing means of understanding. The occurrence, on the one hand, of words which are not at all associated with other words ("basic words", e.g., exclamations) and, on the other hand, of words which take their content from a description by means of other words (symbolic language) may be an indication for tracing out the extreme levels of an analytic gradation. The synthetic construction has as its starting-point simple elements belonging to a higher level of the analytic gradation, but which, from a synthetic point of view, are looked

---

[1] For chronological particulars see also Chapter V of the present monograph.

upon as basic words, the original description of which is left out of consideration. The synthetic gradation is therefore artificially formed with a special aim and by utilizing special (physical, psychological, biological or other) material.[2]

The present program of the *International Society for Significs* pursues theoretical as well as more practical aims; it continues the research into the theories of the foundations of science and aims at the construction of an efficient scientific terminology; furthermore it promotes the empirical study of group languages (content analysis, slogan analysis, bias analysis, etc.).

Just as Logical Empiricism, Significs forms part of a more general critical tendency which during the last fifty years has revealed itself in nearly every domain of culture and of science, and which may be regarded as a reaction against the overestimation of emotional arguments, slogans and metaphors at the expense of critical analysis and observation. The creation of a non-emotional, formalistic terminology with the purpose of facilitating the reproduction of the results of observation, may be seen as one of the means to realize these aims.

However, one should not forget that this remedy does not prove to be useful with regard to the results of observation of social volitional processes which require an analysis of the human means of understanding, taking into account not only the indicative, but also the emotive and volitive elements of the processes in question.

II. Methodology

Significs aims at an investigation of the acts of communication, i.e., of acts by which living beings try to influence the behavior and the activities of other living beings. In a somewhat narrower acceptation it may be described as the scientific study of the mental associations underlying the human acts of communication, excluding the more specific departments of the science of language in the proper sense, like philology, etymology, semantics. For, viewed from a signific angle, the lexical word is not an act of communication, but the outward form common to several of these acts. And where semantics does not leave out the associations of the words with the representations, emotions and impulsions of the "speaker" and the "hearer" (i.e., the individual or group performing the act of communication or influenced by it), it considers them from a general point of view and neglects the individual circumstances. Therefore, the psychological analysis of the lie belongs to the domain of significs, and not to that of semantics. And even though we might argue that *every* act of communication is intrinsically purely subjective, since it is meant to influence other living beings, yet there is a great difference in the amount of satisfaction the acts of communication provoke in the speaker, a phenomenon which may give rise to a twofold distinction of the acts of communication. On the one hand, there is a category, that of the volitive expressions proper, in which this satisfaction is dependent on the choice of the hearer, whereas the other category, that of the descriptive or indicative acts of communication, brings about satisfaction mostly independent of the choice of the hearer. In the latter case, the satisfaction is connected with the degree of perfection of the act of communication itself; the volitive element is strongly repressed and the act is almost exclusively associated with our representations (e.g., the bald description of an object or an event). In this sense, the indicative value of an act of communication can be defined as the associations (in the mind of the speaker and the hearer) of recollections resulting from the observation of objects or events of the physical world — which may be reduced to the perception of resemblance and difference — whereas the emotive value has relation to the other associations, particularly to those referring to emotions and impulsions.

---

[2] See also Chapter II.

From these conceptions results a quite particular acceptance of "signification". In a wider sense, the (signific) meaning (signification) of an act of communication can be described as the influence the speaker aims at exercising or the hearer undergoes (active and passive signification), and also the influences which have given rise to the performing of the act of communication (symptomatic signification). In a narrower sense (signification of a word), it bears on the part a certain word or expression takes in the signification of the acts of communication in which the word or the expression has been used.

This definition clearly demonstrates the difference between the empiric method applied in significs and the introspective one used in traditional philosophic literature. A number of philosophic books are devoted to the study of the "meaning" of terms like "causality," "existence," "truth," "life," etc.; however, most of these books contain nothing else but communications about the verbal associations provoked by these terms in the mind of the authors.

The signification of a word is never fully understood, not simply because it is nothing but the influence expected by the speaker with regard to the actions of the hearer, but also because every individual belongs to different groups and every group has its own "parallelism of will," which is kept up by a more or less specific language (idiom). Instead of attaching a fixed signification to a word, the significist pays more attention to the associations which are roused by the performance of an act of communication. By his analytic approach he tries to find out the mental associations connected to a word-image or a group of word-images (the "psychic correlate"), i.e., the representations or the recollections originating from the hearing, the reading, the pronouncing or the writing of a word (auditive, visual and motorial word-image).

For the main task of analytic significs may be described as the psychological investigation of the *existing* means of understanding. On this level of research the significist does not aim at formulating definitions the contents of which are detached from ordinary usage. On the contrary, at this stage he applies himself to examining and registering the verbal behavior of individuals and groups, considering its regularities and peculiarities, and aims at constructing a gradation founded either on a differentiation of the word-associations he observes, or on the nature and the number of indicative elements of the acts of communication performed by his "subjects," or on some other (e.g., logical) criterion. For the investigator himself such a gradation, whose successive levels may show a gradual increase of the indicative value of the terms introduced, may be a helpful means to get insight into the structure of pseudo-problems and pseudo-judgments (i.e., formulations which have the appearance but not the contents of indicative problems of choice or judgments of choice) and to recognize and to avoid circular reasonings if he wants to build up a scientific terminology suiting the aims he pursues.

The synthetic approach, however, confronts the significist with other problems. It aims, not at investigating, but at improving and extending the means of understanding in use. Although the synthetic gradation may be built on the analytic one, on the understanding that the terms of the "higher" analytic levels become basic terms of one or more of the "lower" synthetic levels, the synthetic definitions include new linguistic means and as such have an artificial character. This may be illustrated by the following example.

In conversational language the word "instinct" often refers to a distinction between the actions of animals and men. No doubt this word is known to the readers of the present monograph, but it did not occur in the vocabulary they availed themselves of in their early youth. They have been acquainted with it by means of other words which they knew already at the time when they became aware of the term, and that not because they have learned a definition of the word, but inasmuch as they have heard it used in connection with the behavior of animals and men. However, the statement "animals are prompted by instinct, man is guided by his intellect" is not an *empiric* statement about animals and men, but a statement about the *usage* about animals and men, and in this form may give rise to all kinds of "terminological" questions and problems. The occurrence of indications or operations in the use of the word would place the term on another level of the analytic gradation.

The synthetic approach could start from the observation that some of the forms of behavior of animals and men, however complicated they may be, are characterized by a "typical" incipient and final stage (e.g., sneezing), whereas other forms show this typical character only in the incipient stage, but differentiate more and more in their final stage (e.g., flying), or conversely show a typical final stage (e.g., clutching at a prey). Viewed from a synthetic angle, the former group of phenomena might be called *instinctive actions* if they are brought about without the agent of observable stimuli, and *reflex actions* if such stimuli are present. The second group of actions (those with a typical incipient stage) might be named *actions of defence*, and the third category (those with a typical final stage) *purposive actions*.

Whether these terms correspond with those of the living language is an irrelevant question here. The main object of the gradation is that circular reasonings cannot creep into the descriptions and that questions referring to "instinctive actions" or "purposive actions" occurring with animals and men are made amenable to an experimental response.

In distinction to the analytic gradation, which is rooted in *extrospective* observation of language (the verbal reactions of others), the synthetic gradation, being an artificial construction, is anchored in the speaker's own language-sense, in his own "nets of associations." To synthetic definitions criteria belonging to the word-family "efficient-non-efficient" may be applied, but not those belonging to the word-family "true-false."

Two other procedures have found acceptance in signific circles, procedures tending to recognize the "dispersion," i.e., the variability of the signification of words and word-complexes with regard to different individuals (*inter*individual dispersion) or to one and the same individual (*intra*individual dispersion): the *method of exhaustion* and the *method of transformation*. The former enables a signific investigation by comparing different situations which may give rise to the "same" verbal reactions, the latter by comparing different expressions aiming at rendering the "same" situation.

The acts of communication can be distinguished in those having a more or less *personal* and those having a more or less *impersonal* character. Impersonal acts of communication, having an indicative or physical value, are conveyable to a wider circle of hearers than emotive and volitive expressions and therefore can be investigated in a more formal way than the latter. The *mathematical-logical* or *syntactic-axiomatic method* lends itself best to studying the most impersonal or "objective" acts of communication, but is not qualified for an investigation into "subjective," emotive expressions. Between the "axiomatic" and the "introspective" level there is a variety of acts of communications which may be characterized by an increase of the personal element and to which a psychological method, like the signific one, can be usefully applied. In this connection it may be stressed that these distinctions, like any distinction of this kind, are artificial, and that the line between objective and non-objective acts of communication is often too sharply drawn: even in the "most objective" act of communication the volitive and emotive elements are not entirely lacking. Signific investigations show that there are impersonal, objective, "physical" acts of communication that have a much greater dispersion than one usually expects. It will be obvious that, generally speaking, a rather small dispersion has to be attributed to *proper names* and *technical terms* and that the use of *value judgments* is attended with a great dispersion of the terms involved, but it may be less obvious why the numerals "zero" and "one", in distinction to "two" and higher, differ in this respect, and why words like "perfect", "nothing" and "mathematics" belong to those having a great dispersion.

Confronting subjects with a number of visual situations (e.g., a photographic or kinematographic exposure) and asking them to interpret the latter, one can observe that it is rather difficult to choose pictures to which the subjects respond by the *numeral* "*one.*" "*A* house," "*a* dog," "*a* horse," may be the interpretation of three pictures which we pick out to that purpose, and only by showing a *series* of pictures (by which the counting is suggested) will one get the desired response: "*one* house," "*one* dog," "*one* horse." It will prove to be much easier to call forth the numeral "two" by the representation of an isolated situation; to this end it will be sufficient to show clearly any similarity, and a picture of "two houses," "two dogs" or "two horses" will probably lead to the desired result.

It is still more difficult to confront subjects with situations (or classes of situations) to which they respond by means of words like "zero" and "nothing." These words prove to be used to indicate a certain difference with regard to other situations, but have in themselves no indicative elements. The same can be said with respect to the so-called "Allaussagen," i.e., statements tending to denote the most general concepts. Apart from their sometimes mainly emotive functional elements, they, too, have no indicative signification and are almost exclusively used in a comparative way.

The method of *transformation* will often be useful to the investigation of the dispersion of the function of a word or of a group of synonymous words (word-family), especially where the dispersion has remained unobserved. This method particularly lends itself to revealing volitive and formal elements of acts of communication; the latter refer to elements bearing on *word*-associations only. One of the most simple transformations which can easily be applied to acts of communication of daily and public life is that from the approving into the disapproving terminology. It will be evident that words like "heroic" or "reckless," "laudable" or "blameable" do not refer to a clearly defined situation but vary according to the approbation or the disapprobation of the speaker. It is not difficult to see either that questions referring to "justice" or "injustice," "freedom" or "coercion" can be reduced to the "positive or negative charge" of the "same" representations of different persons, but it may be less easy to understand that this personal element sometimes plays an important part in all kinds of divergences of opinion of a more scientific nature. Neither speaker nor hearer always recognizes the volitive element underlying many social, political and economical problems, even though the latter refer to situations in which they are involved. Moreover, we all have a tendency to give our valuative, subjective judgments the appearance of distinctive, objective judgments, the latter being more conveyable than the former ("it is . . ." instead of "I think it is . . .").

From the above explanations it follows that statements which do not contain experimental, verifiable observations are not at all immaterial in the eyes of the significist. On the contrary, he regards volition and emotion as the fundamental elements of every act of communication, although he does not disregard the important part the indicative element (the only one which can be related to the concepts of "experiment" and "verification" in a physical sense) plays in more or less impersonal communication.

From a "macroscopic" point of view, both personal and impersonal acts of communication greatly differ in their volitive value. From the beginning, significs has accentuated this aspect of the "problem of the pseudo-problems." For this kind of "pseudo-problems" (*Mannoury* has called them *unreal pseudo-problems*), although they may often have the appearance of indicative, distinctive problems, should be sharply distinguished from those statements which, after being subjected to a terminological transformation, prove to be "lacking any sense". On the other hand, there are problems of a purely terminological nature whose statements can be reduced to formal elements only; to them belong the problems of formalistic mathematics, but they will not give rise to misconceptions, unless their formal character has remained concealed; in other words, unless they are "real pseudo-problems."

In general, real pseudo-problems are not easy to investigate and often require an exact analytic study of the dualistic character of colloquial language ("I-it-transformation") and of the subtle structure of mental processes, the description of which cannot dispense with new linguistic means ("micro-psychological" terminology).

In some cases it may be useful to introduce a micro-psychological, a micro-logical or another micro-terminology — unified and formalized terminologies — as the starting-point for further formalization; in this way endeavors have been made by significists to anchor fundamental mathematical concepts in a micro-psychological terminology.

Especially in tracing the indicative elements of emotive and volitive acts of communication, the operational method, requiring descriptions which take their contents from the concrete operations that enter into the definitions of the concepts employed, has also proved to be very helpful. By giving an ''operational meaning'' to terms like ''freedom,'' ''determinism,'' ''justice,'' ''state,'' etc., these terms get a more or less indicative sense.

The *analytic* signific approach may be best effected in an experimental way and with the aid of many subjects and many observers. To results which are obtained introspectively only a minimum value may be attributed. It goes without saying that the methods described in this chapter should be applied without preconceived opinions. The establishment of the reliability of a ''signific test'' does not differ essentially from the establishment of the reliability of a psychological test or ''type of observation'' in general, although signific investigations in their statistical approach, especially in the domain of values, meet with particular difficulties (see also Chapter III).

III. Applications

The methods and principles set forth in the preceding chapter have found application in various fields of science: in mathematics, in logic, in physics, in linguistics, in psychology, in biology, and in the social sciences. Not only some of the fundamental concepts of these sciences have been subjected to a signific analysis and the usage in different scientific circles has been studied, but also new terminologies more useful than the existing ones from a signific point of view have been constructed. The first part of a signific grammar for the use of college pupils has been recently written.

The application of significs in the field of mathematics is founded on the distinction between *mathematics in a more limited sense,* referring to the verbal expression of the mathematical science, and *mathematics in a wider sense,* bearing on the system of mental associations underlying the outward symbolic appearance (*formalistic* and *intuitionistic mathematics*). The formalistic expression of the ''law of constancy'' (*Mannoury*), being the foundation of mathematics in a more limited sense, is the principle of identity ($a = a$). The signific approach aims at finding out the kernel of the complexes of mental reactions that give rise to those acts of communication which are known as mathematical. This relativistic point of view differs in many respects from the traditional one.

In colloquial language the signification of a term like “infinite” is of an emotive character; in formalized mathematics it is purely formal. On this consideration is based the conception that in the theory of sets those statements referring to *infinite sets* on the one hand and *empty sets* on the other, in other words, the statements which in defining these sets require the *negation of exclusion*, exclude a physical correlate. For terms like “infinite,” “general,” “empty,” “real,” “eternal,” “material,” “I,” belong to a group of linguistic forms, which may be called the linguistic form of *generality*, and which are connected to the negation of exclusion by the formula “a or *non* $= a =$ everything” (principium tertii exclusi) and “a *and* non $= a =$ nothing” (principium contradictionis). In distinction to the *negation of choice*, bearing upon statements with a mainly indicative signification (distinctive propositions), the *negation of exclusion* refers to statements of a mainly emotive, in particular, declinatory character.

Every linguistic level may be called formal as far as it gives expression to the speaker's associations with other *verbal* associations. And *every* linguistic level may be called spontaneous, meaning that the particular form of every act of communication is determined by the state of mind of the speaker at the time when the act is performed. Considering this, one could say that there is not an essential but only a gradual difference between mathematical and non-mathematical language.

“Demonstrating,” “refuting,” “deducing,” and the like are terms which formalistic mathematics has taken from conversational language, without parting them entirely from the emotive and volitive elements characteristic of these terms in colloquial language. Such a mixed language occurs where a branch of science does not rest upon a far-advanced formalization of language and where the signific foundation did not precede the axiomatic one.

Two ways may be followed to define the concept of mathematics. The *analytic* definition could be derived from the usage of the ordinary mathematical terminology. But this terminology varies greatly, not only from individual to individual — the controversy between intuitionists and formalists is to a large extent attributable to these variations — but also with regard to one and the same mathematician. Therefore, let us try to define this field of science in a *synthetic* way. Leaning on our own verbal associations, we can map out the domain of mathematics and compare it with the corresponding empirical contents. In this sense thus, according to our subjective language-sense, we can establish that one speaks of formalized mathematical language when the hearer's linguistic reaction to the speaker's act of communication is as independent as possible of the persons involved and of the circumstances under which the act is performed. It is obvious that this synthetic formulation is characteristic of the usage of certain *groups* of persons and that even in this limitation it contains many more elements than what is usually called mathematics. Many people will reply in a similar way to the question: "What is the first letter of the alphabet?" or to the question: "What is the sum of three and five?", although few people will say that the former question belongs to the field of mathematics. In eliminating the "circumstances," we have excluded the physical language from the domain of mathematics.

The more the signific procedure develops in a synthetic direction and the new linguistic forms get a formalistic character, the more it approaches the axiomatic method. However, from a signific point of view, the deeper significance of mathematics lies in the progress of the analysis to which human language and human thought are subjected. And from this analysis, too, it takes its (relativistic) "general validity."

In this connection some consideration may be devoted to the theory of probabilities. From a signific angle, a distinction should be made between the *mathematical* and the *applied* theory of probabilities. The former can be entirely formalized and axiomatized and, consequently, does not have other signific foundations but the mathematical formalism in general. As to the *application* of the theory of probabilities, however, one should distinguish between the finitistic and the infinitistic theory of probabilities, the former being applied to the concept of "probability," which in most cases refers to expectations concerning future events, the latter differing from the finitistic one, inasmuch as it applies the negation of exclusion in defining the chance-number 0 as a result of limit-transition. The difference between the notions of the finite and the infinite is of a purely emotive character and cannot be defined in terms of (real or hypothetical) experiments. The endlessly discussed question whether $P = \frac{E}{N}$ tends to a limit with N increasing ad infinitum or not arises from the mixing up of those two elements in the meaning of the word.

The mathematical form of thought in a more limited sense distinguishes itself from other forms of thought by a formalistic feature and a more explicit gradation of the significations introduced and by the isolated character of the "net of associations" which is characteristic of this mode of expression. All formal-mathematical systems underlie the (mostly implicit) assumption that from the beginning all uncertainty and all indefiniteness are left out of consideration. The weak point of the "proverbial" form "two times two is four" lies in the untenableness of the arithmetical rule to which it can be reduced: "1 = 1," a sequence of "words," the psychic correlate of which cannot be designated with "mathematical certainty."[3]

Although the *formal* syllogism can be said to belong exclusively to scientific usage, yet this form of reasoning is not alien to common parlance. In the latter case we have to do with a transfer from a more *general* expectation to a *particular* perception ("all negroes are unreliable," "this man is a negro," "consequently, this man is unreliable"). But also the reverse phenomenon occurs: the extension of a particular emotion to a more general one (major: "This man is unreliable," minor: "this man is a negro," conclusion: "all negroes are unreliable"). Expressing ourselves in a signific terminology, we can describe these processes of deduction and induction as *mental phenomena of pulsation aiming at translating emotive*

---

[3] A striking similarity of views we find in the writings of *P. W. Bridgman* and in those of *Arthur F. Bentley.* "We have come to expect by now that when we push our analysis far enough we can uncover imprecision and uncertainty. This is also the case with the human enterprise of logic. Consider a simple syllogism in the conventional form 'If all a is b and if all b is c, then all a is c.' In this syllogism the same symbol, 'a' for example, occurs twice. Now 'a' stands for something — the fact that the symbol occurs twice implies that it makes sense to speak of the referent of 'a' remaining the 'same' during the progress of our discourse. What does it mean to say that the referent remains the 'same' during the discourse, and how shall we check on any tentative referent to insure that it really has remained the same? 'Sameness' is by implication a word of absolute precision, and we know that we never experience such. It must be that many ordinary referents have the property of 'sameness' to a sufficient degree to justify their use in a syllogism. But what is the criterion that they possess it to a 'sufficient' degree except the criterion that the syllogism gives the correct result when we assume it? I think there is no other answer and that the syllogism always conceals a certain degree of circularity". (*P. W. Bridgman,* "Some Philosophical Aspects of Science," paper to be published in *Synthèse.*)

and indicative experiences and recollections into expectations of a more narrow or a more wide bearing (the "process of regrouping," *Mannoury*).

It is true that the development of the symbolic and pasigraphic method has enabled us to bring essential parts of classic and more modern systems into a deductive form, but this does not imply that for the significist the fundamental difficulties resulting from the formalizing of "living language" are now solved. He has observed the phenomenon that the "higher" logical linguistic levels lose their suggestive power, i.e., their usefulness with regard to the conveying of mental contents, according as they increase in regularity. From what has been said it results that the main signific problems lie here in the zone of the transition from conversational language to formalized language, and vice versa (the "switching in" and the "switching off" of a formalistic system). Questions like "How far is it possible to express mathematical thinking by means of pasigraphic systems?" and "Which are the foundations of such a symbolization of mathematical thinking? ", which have been raised in signific circles, confront the significist not only with problems of observation of language, but also with those of experimental psychology.

It may be said that *Brouwer's* mathematics, by analyzing and limiting the validity of the principium tertii exclusi, has made mathematics applicable to "spheres of life" which have been unaccessible to classic logic and that this mathematical intuitionism, even in its formalized expression, is much more close to living language than the logistic system of *Peano-Russell.*

In the domain of physics signific studies so far have mainly concentrated on the analysis of some fundamental concepts, such as "time," "space," "motion," and on problems of a more epistemological nature: "causality," "continuity," and the like. From a signific viewpoint, the notions of causality and physical law are nothing but the general and particular expressions of the regularities representing in most cases the associations recollection of expectations — observation (*empirical contents or indicative signification of the physical laws*), and, on the other hand, the corresponding regularities of the associations recollection of observations — expectation (*emotive and volitive signification of the physical laws*). An *objective* content, in the strict sense of the word, i.e., a content independent of man, could not be attributed to physical laws, inasmuch as they are in the last resort psychological laws; they exercise a great influence on human life also because their knowledge facilitates the transition from "end to means" (*Brouwer*).

The problem of causality refers to the question as to the psychic correlate of the family of words to which terms like "cause" and "effect" belong, and a signific investigation into these terms shows that they do not contain a more or less definite complex of representations or affections, but that they consist of what may be called a network of associations or correlations which find expression in conversational language by words like "in consequence of," "by so doing," "because," etc. In the most striking cases the causal "ordinative" principle bears on associations between sequences of recollections and sequences of expectations of an *indicative* nature: I remember having seen a certain sequence of phenomena and expect to observe the same sequence of phenomena in future. Wondering what, from this point of view, is the signification of what one calls the *general* principle of causality or the *general* law of causality, we will reduce this principle to a twofold empirical rule, expressing on the one hand that *my* expectations (and, as far as I can see, also those of my fellow-men) usually more or less agree with a certain average of resemblant sequences of images of recollections, and, on the other hand, establishing that, as far as I remember, my experiences in the past were in agreement with what I had expected a moment ago ("law of regrouping" and "law of prediction," *Mannoury*).

It is evident that this signific interpretation differs greatly from the absolutistic conception speaking of an "unshakable" and "generally valid" regularity to which all phenomena inexorably are submitted, and that the difference between the two conceptions can only be expressed in emotive terms. The question whether the above principle of causality has a general validity or not is regarded by the significist as a purely terminological question.

It goes without saying that the concept of "matter," of "lifeless substance," as element of a conception of the world, has also been subjected to signific criticism. It has been replaced by the representation of a sequence of phenomena or *events* which, from a human point of view, may be regarded as elementary, and attempts have been made to formulate a theory according to which spatial and temporal relations are made subservient to those between the events, the "flashes" (*Van Dantzig*).

However, the influence of the American operationism has left its traces in signific circles these last years, and some of the conceptions prevailing there can be rendered by the following quotation taken from *Philipp Frank's Modern Science and its Philosophy* (p. 295): "From a psychological viewpoint Einstein describes this way of producing theories by free imagination in a letter to the French mathematician Hadamard. According to this new conception, it is true that physical theories are the product of free imagination, if we take the word "free" with a grain of salt. But it must not be concluded that these theories are products of metaphysics. For these theories are subjected to the operational or experimental criterion of meaning, though in a more indirect and complex way. The criterion of truth remains ultimately with the checking by sense observations, as the older 'positivists' claimed. But we know now that this checking is a more complex process than it was believed by men like Comte and Mach to be."

Carnap has defined the "meaning" of a statement in science as the sum of all statements about similarity and diversity between sense impressions that can be derived logically from the statement in question. (*The Logical Structure of the World.*) "Operational definitions" enable us to derive from the symbolic system observational facts that check with our actual observations; they are the descriptions of the operations by which abstract symbols (e.g., the space-time co-ordinates) are connected with observational phenomena. According to *Bridgman*, a theory which does not contain the operational definitions of its abstract terms is "meaningless". Like the significists, *Bridgman* pays attention to the *circumstances* under which acts of communication are performed, and we may say that his conceptions and views have been most favorably received in signific groups as a means to avoid confusing discussions about theory construction, also in the field under consideration. That the structural definitions are bound to logical and practical limitations, has been particularly stressed by *Clay*, who has pointed out that in the development of science there is not only a regression of the *unstructural* on micro-levels of observations, i.e., the possibility of giving structural definitions of micro-processes, but that there is also a regression, in the opposite direction, of the *structural*, i.e., of the possibility of describing a macro-complex as an element of a more complicated structure.

In biology the object of significal investigations has been from the beginning the critical study, both from a logical and from a psychological point of view, of the fundamental concepts of the various departments of this science. It was evident to the significists that many of these concepts, especially those occurring in speculative biology, urgently need such a study, as emotive and volitive elements are often abundantly represented in the terminology of speculative biology. The recognition of these elements is essential for a clear understanding of many problems in this field; their elimination, by an attempt to formalize the concepts used, was considered to be a useful means to avoid endless and fruitless controversies on pseudo-problems.[4]

It may be stressed that the object of these activities is the *study*, not the fixation of biological concepts; their purpose is informative, not normative. Science is like a living and growing organism; its conceptual apparatus is developing with its progress. A premature fixation of the meaning of concepts would inhibit this necessary development. Therefore, one has to restrict oneself to the attempt to determine and to formulate as accurately as possible the meaning of a concept at the time being; to recognize its eventual irrational elements; and to eliminate these elements, as far as possible, by a formalization of the concept. It is essential that the way in which the concept can be applied to the empirical objects of science be clearly indicated, and that the further development of the concept is given space.

Instead of subjecting some concepts of speculative biology to a critical analysis, e.g., those of "wholeness," "plan," "design," "entelechy," etc., the biological section of the group of significists followed another procedure. The nature of the phenomena to which most of these fundamental concepts of general biology refer can only be understood if agreement exists about the meaning of other notions belonging to the field of empirical biology. It was necessary, therefore, to begin with the study of these concepts of less wide scope, but more immediately related to the empirical data of biology. Only when, in this way, the fundamental concepts of the various branches of empirical biology have been examined, a sufficient basis is formed, from which one may endeavor to get an understanding of the fundamental questions and notions of general and speculative biology.

---

[4] See also the general survey of the activities of the Biological Section of the International Society for Signifies, by *Chr. P. Raven*, *Synthèse,* Vol. VII, Nos. 1-2, p. 93 ff., from which the particulars of this part of the monograph are taken.

The study of the concepts of *descriptive* biology had to be preceded by a discussion of description itself. The relations between reality, perception, observation and description were discussed. Every description is adapted to a certain purpose, and does not satisfy other purposes; no description can be complete in itself. The elements of description are the properties imputed to the objects described; they can be classified as quantities, qualities and arrangements. In these discussions a remarkable difference with regard to the views on the relationship between the concepts of "property" and "characteristic" came to light. The biologists were inclined to define a "characteristic" as a special property, being important enough to be used for the recognition of the object to which it belongs. For the physicist who took part in the discussions a "characteristic" was a superficial feature of the object, and had nothing to do with its "properties," the latter concept denoting the fundamental elements of its nature. Evidently the difference in significance attributed to these words is due to psychological factors; they have not further been studied.

In biology description makes use of biological notions at first; with the progress of analysis, these are reduced more and more, however, to arithmetic, geometric and physico-chemical notions. The properties of arrangement or structure are very important in biology; spatial and temporal structures may be distinguished. With regard to spatial structures, the problem of elementary units in biology and the concepts of unity, individuality and wholeness were discussed. The study of temporal structures involves the concepts of causality, law and rule, whereas a consideration of the spatio-temporal structural relationships as a whole brings to the fore the concepts of design and sense. "Causality" and design do not exclude one another, but are complementary.

After ample discussions on the concepts of *systematics, species, racé, phaenotype* and *genotype* and on some fundamental concepts of comparative anatomy, the part which the concept of *type* plays in anthropology (e.g., racial and constitutional types) was examined. It was argued that a system can never be a complete expression of phylogenetic relations, as the latter have another dimension, including the factor "time." The resemblance between the hierarchic grouping of classification and the genealogical tree of phylogenetics is only a superficial one; it may seduce to overlook the essential differences.[5]

These discussions were followed by a critical examination of the concepts of those parts of biology that study the vegetal, animal and human communities. Only part of this program has been completed by now, as only the concepts of phytosociology have been discussed. The notion of "plant community" implies the existence of mutual relationships between the members of such a community; they can be summarized by the concepts of "competition" and "priority," and give rise to a social structure of the community. The concept of "competition" was defined as the mutual influence between equivalent plants having similar necessaries of life, thereby restricting each other in their ability of expansion. Whereas a "plant community" is an abstraction, the concrete samples of vegetation may be indicated as "populations" or "phytocenoses"; they are distinguished by the fact that phytocenoses possess a social structure, whereas this is lacking in a population. It was emphasized that the word "phytosociology" is misleading, as fundamental differences exist between the plant communities and the communities (societies) of man and higher animals, which are studied by sociology.

In the course of these discussions many concepts of a more general nature, like those of causality, probability, chance, finality, order, purpose, wholeness, have been touched upon. The existence of three leading ideas in biology has been stressed on many occasions: 1. the causal idea, bearing on the relations between mentally isolated events; 2. the historical idea, relating the phenomena to the single irreproducible series of real events called history; 3. the final idea, bearing on the relationship of the phenomena within a living, a biological, system to common space and time coördinates. It may be expected that it will be possible by clearly distinguishing between these complementary and equivalent ideas, to attain a certain degree of formalization of many of the fundamental concepts of biology.

"Significs makes a study of the effects on human behavior of the linguistic aspects of the evaluative process, the most distinctly human aspect of the behavior of the human organism."[6] This definition, given by *Anatol Rapoport* at our Fifth International Signific Summer Conference, certainly bears on one of the fundamental aspects of the signific approach. It makes clear why the significists are particularly interested in some fields of inquiry which have implications for their special discipline. As such may be mentioned *Cybernetics, Mathematical Biophysics* and the *Mathematical Theory of Communication*, in which, as in significs, the word communication is used in a broad sense to include all the procedures by which one mind may affect another. Two of the three levels at which *Warren Weaver* considers the general communication problem, viz., the level referring to the question, how precisely the transmitted symbols convey the desired meaning (the semantic problem), and that bearing on the problem, how effectively the received meaning affects conduct in the desired way (the effectiveness problem), are specific signific problems.[7]

---

[5] For further details, see *Chr. P. Raven,* l.c.

[6] *Synthèse,* Vol. VIII, Nos. 3-5, p. 193.

Starting from a simplified schema of cellular occurrences, *mathematical biophysics* aims at mathematically deducing experimental results in the field of higher mental functions with considerable specificity and accuracy. The understanding of the events in the nervous system and analogous systems as determined by their structure is claimed to be fundamental for the understanding of abstraction, evaluation and communication — processes of basic importance in significs. On the basis of *Rashevsky's* picture of the nervous system it would be possible to describe quantitatively such relations as reaction time as a function of the interval between a preparatory "warning" and the stimulus; acuity of discrimination as a function of the magnitude of the stimulus, and many other relations. The approach may also be useful in suggesting "models" to account for a wide variety of "psychological" phenomena, such as conditioned reflexes, error elimination (as observed in maze learning), logical "thinking," visual perception and esthetics, and some psychotic states. The theories regarding the construction of electronic computers and calculating machines from the point of view of invariances involved in such devices have been received in signific circles with vivid interest. And this is obvious. The study of the linguistic aspects of evaluative processes — one of the items of the signific program — is closely connected with that of the events associated with evaluative processes, which mathematical biophysics seeks to describe in physicomathematical terms, and also with the invariants common to these other processes, which cybernetics try to discover. The words of Weaver that "one has the vague feeling that information and meaning may prove to be something like a pair of canonically conjugate variables in quantum theory, they being subject to some joint restriction that condemns a person to the sacrifice of the one as he insists on having much of the other," have not escaped our attention.

---

[7] *Claude E. Shannon* and *Warren Weaver*, "The Mathematical Theory of Communication," The University of Illinois Press, Urbana, 1949.

In this connection special mention may be made of *Charles Morris'* semiotic with its main subdivisions: *semantics, syntactics* and *pragmatics.* The reader of the present monograph who is acquainted with Morris' science of signs will have noticed the similarity between his conceptions and those of the significists.

In the field of *sociological* studies, taken in the sense of scientific studies of the phenomena arising from, and conducive to, social interactions and interrelations, significs has also to accomplish the double task of clarifying the conceptual framework of existing terminologies and of providing the sociologist with a procedure for the construction of his theories; in other words, to furnish the means for a foundation of a scientific sociological terminology. Sociologists often do not express themselves clearly, because they neglect the task of introducing exact definitions and making linguistic analyses, and, in default of this, avail themselves of abstractions devoid of all indication. Operational definitions, dealing with concepts which are construed of operations, have proved to be a useful means to link abstract words with our experience.

According to the significists, language is one of the most important social factors; it is underlying every study of the individual and the group, of the family and other social organizations, science, jurisprudence, art and war. Sociology in itself and by itself gives a strong impetus to significs by studying the conditions under which signs and symbols have a "meaning"; by examining human responses to signs and by examining the relations between language and human behavior.[8] The connection between sociology and significs is evident. The significist has learned from his observations that often differences of a terminological nature separate different conceptions: in politics, in sociology, in philosophy, in the theories of the foundations of science. Each trend develops its own usage, its own idiom, which easily gives rise to misinterpretations by other trends. Moreover, not all authors distinguish clearly between their own opinions and emotions and those they ascribe to members of a community.

---

[8] This is also an item of the program of the General Semanticists, whose writings have been and are being carefully studied in some of our groups. In Great Britain similar studies have been undertaken by the jurist *Alvin C. Leyton* (London).

In the social sciences signifies has been applied partly to sociological techniques and study methods and to the recording of observations of language, partly to investigate the relationship between language and human behavior, especially the influence of language on the creation of beliefs, ideals, and aspirations. For that purpose significs aims at studying problems with regard to social control: the several types of norms regulating and controlling behavior (convention, fashion, religion, morals, law), and at investigating the social processes, the various forms of interaction between individuals and groups, including social differentiation and integration.[9]

The signific approach places before the sociologist the question whether the acts of communication he is investigating bear upon a concrete situation or not. For the main aim of his investigations is to record the verbal or non-verbal responses of individuals and groups to verbal or non-verbal stimuli, and to analyze and to classify them. If the significist is not able to confront his subjects with observable situations, or in general to record reactions to concrete situations, he will have to get on with collecting and analyzing texts, quotations from the press, radio, etc., referring to the situation in question. This material might be the expression of the representations of a certain individual or a certain group, but might also be the symptom of a certain state of feeling or disposition of the individual or the group. In this case he is only indirectly in a position to choose his subjects; he does not know whether the groups he is examining are homogeneous, or in which respect they are homogeneous, and whether the specimens, especially if they bear upon judgments of value, are representative of the whole group. His task is to trace the correlations existing between the usage of ideological, political or other groups and the social and political aims predominating in such groups.

---

[9] Basic research of this kind has also been carried out by the Institute for Social Research at Oslo. The purpose of the Institute is to conduct and encourage coördinated research in the fields of Sociology, Social Psychology and Social Anthropology.

He should beware of the danger of examining a single emotive term, for a signific investigation of emotive terms should embrace the whole of interrelated terms bearing upon the concepts to be investigated and should not confine itself to one or some terms which may be easily used as slogans.

Failing a concrete situation the significist has to develop a special technique. Distinguishing different linguistic levels — a linguistic gradation which, as we have seen, is characterized by the fact that words and expressions of a certain level (which of course is not the primitive level) can be replaced or described by terms of the preceding level — he may ask the question: on which level the act of communication he is investigating can be placed. In general it may be said that this will depend upon the amount and the nature of the indications the act of communication contains or the operations to which it refers; the conditions under which the terms are used and the state of development of the language-sense of the subject.

In order to trace the psychological background of acts of communication, the mental associations which are characteristic of their use, the significist will study: (1) the circumstances under which the words are used; (2) the elements and the character of the groups who avail themselves of them; (3) the purpose(s) these groups have in view by using these words; (4) the circumstances under which they avoid explicitly or implicitly the use of certain words or expressions; (5) the social, ideological and political conceptions prevailing in these groups; (6) the question whether the words belong to the ''group premises,'' i.e., to the net of associations that is basic to the judgments accepted implicitly or explicitly by the group; (7) the more or less emotive, volitive or indicative character of the words; (8) the variability of the signification (dispersion) of the expression and its positive or negative emotive value according as it is used by a group or by another (adversary) group; and (9) the operational contents of the act of communication, i.e., the actions which it describes or to which it refers. Certainly, such investigations are long-winded, but urge themselves upon those investigators who will not be satisfied by quasi-scientific results.

To that end one can make use of questionnaires. But in this case one should observe the fact that purely verbal results will be easily obtained, since usually the questions do not confront the subjects with concrete situations and often contain emotive expressions. More often than not, the words used in questionnaires not being analyzed beforehand, the questions anticipate the results of the inquiry and neglect the non-verbal responses of the subjects. The means introduced to parry this inconvenience by ''cross checks'' — mostly of a verbal nature too — at best only partly succeed in breaking through the chain of verbal associations. In this connection it may be noted that material conflicts — conflicts of interests — often screen themselves behind ideological conflicts.

The results of sociological investigations are frequently influenced by subjective appreciations of which the investigators are not conscious, but which may give rise to partial judgments. In particular they should beware of the procedure to trace certain properties of any fragment of a population and to attribute these properties to parts of the population which have not been investigated. Viewed from a taxonomic angle, generalizations of this kind should not be based upon a specimen which is not representative of the group under examination. Sociological studies should clearly distinguish moral appreciations, philosophical theories, and scientific facts. Their records should mention how individuals and groups behave and not how they ought to behave. Signific studies of the impact of the language of a group upon its members, particularly of industrial and political groups, have revealed much of the source of disintegration within society, and of conflicts, actual and potential.

The theoretician's own verbal behavior is often included in the ''material'' he investigates. If we call object-language the language used in verbal behavior of people which sociology investigates and meta-language the language sociology uses in its discourse about the verbal behavior of people, it becomes fairly obvious that in present-day sociology the two languages are still inextricably mixed together. On the other hand, people whose behavior is being investigated often use terms and meanings of the meta-language together with those of the object-language. The sociological meta-language thus becomes a meta-meta-language, and so on.[10]

All these investigations form part of analytical significs, of extrospective observation of language, the principal aim of which is to investigate the current usage. However, when building up a synthetic vocabulary, the social scientist has to face other problems, but, apart from the specific character of the subject-matter, these problems do not differ from those referring to the construction of a synthetic gradation in general. For scientific sociology the construction of such a useful terminology will be an indispensable although laborious task requiring the collaboration of a staff of specialists in various fields. For one should not forget that scientific method depends on the refinement not only of our physical tools but also of our conceptual tools, not only of our experimental apparatus, but likewise of its language of statement and definition.

---

[10] See also *Vladimir Cervinka*, ''A Dimensional Theory of Groups,'' *Sociometry*, Vol. 11, No. 1-2, 1948 (Reported by *Stuart C. Dodd*), and *Vlad. Cervinka*, ''Factor Analysis,'' *Statisticky obzor*, roč. 1948, č. 2.

Considering the foregoing explanations it may be clear that *psychology* is playing an important part in signific studies. The conceptual framework of significs is to a large extent a psychological one. The significist avails himself of current psychological concepts, like emotion, volition, representation, expectation, recollection, impulsion, thinking, believing, etc. If significs enters in this way the scope of psychology, psychologists and psychiatrists, on the other hand, are confronted day after day with signific problems. In the relation psychologist-subject or psychiatrist-patient the apparatus of communication fulfils a considerable, if not a prevailing, function, while the kernel of the signific study, on the analytic side, lies in an investigation of the elements underlying communication. In psychology many terms have as their referents complicated actions, habits and dispositions; for this reason they can be introduced only as a starting-point for the analysis of complex behavior. Moreover, vernacular and technical terms are often confused. In front of this situation significs aims at constructing a micro-psychological terminology for describing phenomena of observation, a terminology which, in describing the processes of auto- and hetero-introspection, will take care of finer distinctions and consequently, in this respect, will be more useful than the current one. The question has been raised how far the vernacular may be efficiently used in theories aiming at predictions of behavior in unstandardized complex situations of "real life," since on higher levels of precision and reliability the undesirable properties of the vernacular tend more and more to obscure statements and make partial elimination of them necessary.[11]

---

[11] See also *Arne Naess,* "Notes on the Foundation of Psychology as a Science," Mimeographed Copy, Oslo, 1948; and *Egon Brunswik,* "The Conceptual Framework of Psychology," The University of Chicago Press, 1952.

On the macro-psychological level operational definitions are introduced to give an "operational sense" to the vernacular terms, which are used to describe the correlations between mental and linguistic phenomena. On the other hand, the significist tries to translate metaphoric descriptions, so abundant in psychiatric and psychological literature, into behavioristic ones or, in fewer cases, to translate descriptions formulated in the it-language into those in the I-language. Records in this field, even those made by experts, are written too often on a level of language lying far above that of the vernacular and contain terms which, unjustly, are being supposed to belong to the idiom of the "hearer" and to be "understood" by him, although they are not rooted in indications or operations.

Besides, within the scope of signific studies, psychologists have been trying to trace the characteristics of the language of psychotics and neurotics comparing them with those occurring in acts of communication performed by "normal" adults and children under similar circumstances; among others with the aim of getting insight into the origination and the development of pseudo-problems and pseudo-judgments. To that end they study the way in which statements are verified in pathological behavior and in which both pathological and normal behavior are justified. In this connection special attention has been paid to the use of metaphors and generalizations (Allaussagen). The problem whether it would be possible to give the concept of "psychical strata" a more indicative signification by relating — as far as the conscious levels are concerned — certain mental phenomena to certain linguistic levels, a research requiring many observations of linguistic behavior, is one of the items still under discussion.

IV. Concluding Remarks

Logical Empiricism, Operationism, Pragmatism, Semantics and Significs — they all have tried to set up criteria by which one can distinguish clearly between "meaningful discourse" and "empty talk." Like pragmatism and operationism, significs lays stress on the conception that the "meaning" of a statement is characterized by the way in which it influences the behavior of men who are trained to react to the language in which the statement is formulated. Logical Empiricism, like significs, stresses the importance of the analysis of our language tools for a comprehensive "scientific attitude."[12]

The different approaches to language- and concept-criticism of the last decades may be roughly divided into an analytic one, an axiomatic one and a signific one. *Bentham, Ogden* and *Richards* may be regarded as the authors who have inaugurated the analytic area; who have pointed out the variety and variability of the significations in colloquial language; and who have shown that the mixture of mainly emotive and volitive elements by which the language of everyday speech is characterized is not suitable to the purpose of useful communication.

The second trend of present day criticism of concepts we have in view, the axiomatic one, finds its exponents in those scientists who do not confine themselves to the "negative," analytic side of the criticism of language, but who aim particularly at constructing logical systems, where possible in a pasigraphic form. Among them are logical empiricists who, just as the significists, have paid special attention to epistemological problems and have subjected the current philosophical theories, abounding with metaphors and generalizations, to a sharp criticism.

We have another aspect of similarity in view when we mention here the operational and the pragmatic approach to "meaning," and in connection with this, the General Semantics Movement, for whom the study of symbols and the inner reactions to symbols are important objects of study.

Both the logico-empiristic and the signific movement have empirical tendencies, although they have not developed the same procedures, even with respect to their criticism of language.[13] The substitution of philosophy by a logical analysis of language is not one of the items of the signific program, which aims at clarifying the "problems of life" by modern psychological means. The significists do not exclude from their discussions problems belonging to the "domain of will," but they try to reduce them to their psychological elements. They have not drawn up an *index verborum prohibitorum,* based on the refusal to admit certain terms and expressions to science and philosophy. Both movements aim at unmasking pseudo-problems and at building up an efficient terminology. However, the significists, availing themselves of psycho-linguistic means, distinguish between "real" and "unreal" pseudo-problems; after being subjected to a terminological transformation the latter prove to have an emotive or volitive "meaning" (conflicts of will). Both the logical empiricist and the significist assume that one of the causes of the careless and naïve usage in science is the inaccuracy of common language which, among others, leads to the temptation to substantialize many terms. The significist especially emphasizes the dualistic and mixed character of colloquial language and the value of introspection as a scientific method aiming at recognizing subjective mental elements which, however, can be objectivized ("intersubjectivized") by comparing the introspective results obtained by a staff of scientifically skilled introspectors. This should not be misunderstood. With regard to analytic significs, which aims at registering, studying and analyzing the linguistic behavior of "others" (heterospection), introspection can have only a very restrictive, and mainly comparative, value, but in the synthetic signific approach, the construction of a new terminology, the introspective element occupies a prominent place. And the investigation of these introspective elements underlying acts of communications on synthetic levels belongs also to the field of signific studies. By claiming a multilateral elaboration of the results of investigations (i.e., an elaboration by individuals belonging to different ideological and social groups) the significist also tries to satisfy the condition of "objectivity," in the sense of "intersubjective observational reliability." In other words, he tries to get classes of responses yielding maximum reliability coefficients between individuals facing common "geographic" situations or situational elements.[14]

---

[12] The history and the development of Logical Empiricism and its significance for present-day science has been clearly described by *Philipp Frank* in his book "Modern Science and its Philosophy", Harvard University Press, Cambridge, 1949.

[13] See also my article "The Psycho-Linguistic Movement in Holland," *Philosophy of Science,* Vol 18, No. 3, 1951.

The signific method may be called an empirical method, as it is based on observation. It tries to penetrate the psychological background of the linguistic behavior; *as such* it does not study, as semantics (in the logical sense of the word) does, the *form* of semantic antinomies, the methods of arithmetization, the theorem of indefinability, and the like. Logical empiricism aims either at a logical analysis of the scientific language (*Neurath, Carnap*) or of all forms of language (*Frank,*

---

[14] Cf. also *Egon Brunswik,* op. cit., p. 11.

Schlick, Von Mises, Morris). According to the significist, criticism of language is not synonymous with logic of science. He aims at analyzing the functional elements of acts of communication, although he does not emphasize the difference in logical grammar of each of the forms of language.

Much of the clumsiness and confusion of argument can be removed by formalization. But significists do not lose sight of the fact that one can tell highly speculative stories by means of symbols and that sometimes a symbolism may even conceal the ambiguity of certain explanations and distract the attention of scientists who are accustomed to rely on symbolic argument. Moreover, in many cases it will be more efficient to investigate *why* and *how* the words are used; in other words, to consider the circumstances and the influences of environment under which acts of communication are performed, rather than the communication itself (*symptomatical signification of an act of communication*). The study of non-verbal responses to non-verbal or verbal stimuli, the investigation of which requires a special technique, therefore forms an integral part of the signific program.

In this chapter we have tried to summarize the empirical and critical tendencies of the signific movement and to indicate the similarities and differences of these tendencies with regard to those of some related critical movements. Present-day significists acknowledge the necessity of devoting their attention, in addition to concept and content analysis, to the improvement of the means of understanding in use, both in science and in daily life. They do not deem the analytic method to be of less importance than before, but the synthetic application of the signific principles to the various "spheres of life" has come more and more to the fore. Notwithstanding its somewhat heterogeneous structure, entailing differences in accentuation which tend to guard against one-sidedness, the signific movement strives after unity of method and the cultivation of attitudes of thought such as will tend to unity.

V. Historical Development

The term "significs" was introduced by *Lady Victoria Welby* to denote "the study of the nature of Significance in all its forms and thus of its working in every possible sphere of human interest and purpose."[15] In an article, "Sense, Meaning and Interpretation,'' that was published in the British journal *Mind* in 1896, in her book *What is Meaning?* (1903) and in her monograph ''Significs and Language'' (1911) she advocated a psychological and sociological investigation of the concepts which at that time dominated in philosophic and scientific discussions. Although she made use herself of a rather vague terminology, yet there are to be found in her writings, apart from their tendency, some ideas which have strongly influenced and stimulated the activities of scientists in western Europe who in the early years of the twentieth century were impressed by the uncritical use of linguistic means in science and philosophy. The following examples taken from her above-mentioned book may be characteristic of her point of view:

---

[15] "Significs and Language," Preface.

> ''There is, strictly speaking, no such thing as the Sense of a word, but only the sense in which it is used — the circumstances, state of mind, reference, 'university of discourse' belonging to it. The Meaning of a word is the intent which it is desired to convey — the intention of the user. The Significance is always manifold, and intensifies its sense as well as its meaning, by expressing its importance, its appeal to us, its moment for us, its emotional force, its ideal value, its moral aspect, its universal or at least social range'' (p. 6).
>
> ''It must be remembered that Significs implies in more than one 'sense' a careful distinction between sense, meaning and significance. . . . From this point of view, the reference of sense is mainly instinctive, of meaning volitional, and of significance moral; we have a sense of discomfort, a thing is true in a certain sense, we mean (i.e. intend) to do something, and we speak of some event, 'the significance of which cannot be overrated.' '' (p. 46).

These notions of ''sense,'' ''meaning'' and ''significance'' were, somewhat liberally, interpreted by the Dutch jurist and significist *Jacob Israël de Haan* in his doctorate thesis on Juridical Significs and its Application to the notions ''liable, responsible, accountable'' (1916).

In 1896 a prize was offered by *Lady Welby* for the best treatise upon the following subject: ''The causes of the present obscurity and confusion in psychological and philosophical terminology, and of the directions in which we may hope for efficient practical remedy.'' The donor of the prize desired that general regard be had to classification of the various modes in which a word or other sign may be said to possess 'meaning', and to corresponding differences of methods in the conveyance or interpretation of 'meaning.' The committee of award would consider the practical utility of the work submitted to them as of primary importance.

The prize was awarded to *F. Tönnies,* professor at Kiel, for his ''Philosophische Terminologie in psychologisch-soziologischer Ansicht,'' in which he showed the mainly subjective character of the usual philosophical terms and advocated the foundation of an international organization for critical studies. Although *Lady Welby* died before the journal *Mind* organized a ''symposium'' on the ''meaning of 'meaning' '' about 1920, it may be said that her views have largely contributed to paving the way for this semantic and epistemologic approach to linguistic phenomena.

The Dutch psychiatrist, author, and poet *Frederik van Eeden* would become the connecting link between *Lady Welby* and the later significists in the Netherlands. At a psychological and psychiatric congress at London in 1892 he had insisted on a clarification of meaning, and this pleading for an investigation of the concepts used in psychology and other fields of science induced *Lady Welby* to get in touch with him. A series of epistemologic studies, to which belongs a remarkable essay on the ''logical foundation of mutual understanding'', in which the principle of gradualness was applied and a distinction was made between the volitive, emotive and indicative content of fundamental terms and concepts, was written by *Van Eeden* during the years 1893-1897. Statements like ''The words of mathematics are pure symbols'' and ''Physical science aims at ordering and connecting our sense perceptions'', occurring in the above essay, anticipate the development of the signific approach to the foundations of mathematics and physics in a relativistic and psychological direction.

The first *group* of people concerned with significs in the Netherlands met during the first World War in 1915, under the startling impression of the misleading slogans with which this war was waged and against which there seemed to exist no other remedy but general philosophical reflexion, which as a matter of course had to be initiated in a neutral country.[16] With that object in view a committee was formed. During its deliberations differences as to the plan of action became accentuated, in the end leading to a schism. The majority founded the *International School for Philosophy* at Amersfoort; the minority thought that durable humanizing of mankind could only be attained if in the first place, besides material, also *spiritual forces and values* could be introduced into mutual understanding in an indicative and objective mode, notwithstanding their poetical and emotional origin.

This minority hoped to obtain results in the desired direction by the foundation of an *International Academy for Practical Philosophy and Sociology.* In a prospectus the following task was assigned to this Academy:

"1. To coin words of spiritual value for the languages of western nations and thus make those spiritual values enter into their mutual understanding (what could be called a 'déclaration des valeurs spirituelles de la vie humaine').

"2. To detect and combat such elements in the present system of law and in the productive activity growing under its protection, as chiefly repress or dim spiritual tendencies, and to propose appropriate limitations for the sphere of influence of law and technics.

"3. To point out and brand those words of the principal languages, which falsely suggest spiritual tendencies for ideas ultimately originating in the desire for material safety and comfort, and in so doing to purify and to correct the aims of democracy towards a universal commonwealth with exclusively administrative function."

In the prospectus this task is further explained in the following way:

"The undersigned are well aware of the fact that such a task as assigned by them to the International Academy for Practical Philosophy and Sociology has been taken up several times by philosophers individually. However, they are convinced that precisely in consequence of the individual character of the work of those philosophers their words could be efficient only for memorizing the expressed thoughts in the minds of the writer and his isolated readers, but never could find a place in the mutual understanding of the multitude and therefore had only a slight social influence.

---

[16] See also *L. E. J. Brouwer,* "Synopsis of the Signific Movement in the Netherlands," *Synthèse,* V, Nos. 4-5.

"They hold the opinion that when the same task could be undertaken in common by a group of independent thinkers with subtle and pure human feeling, their thoughts *formed in the mutual understanding of their circle*, would necessarily find a corresponding language, *allowing them to enter into the mutual understanding of the multitude.*

"Finally, as regards the realization of the proposals of the Academy, it must be kept in mind that a thought, in its quality of embryonic deed, has a far greater possibility of development when it is the common intimate conviction of a group of human beings than in the case of its belonging to one individual only, however courageous that individual may be and however numerous the company of half-understanding followers who surround him."

One of the signers was *Frederik van Eeden;* the other signers were the mathematician *L. E. J. Brouwer*, the sinologist and man of letters *Henri Borel*, and the social worker *H. P. J. Bloemers* (later on burgomaster of Groningen and Arnhem). Soon after, this group was joined by the mathematician *G. Mannoury*, the physicist *L. S. Ornstein*, and by *Jacob Israël de Haan.* The group of seven thus formed founded an *International Institute for Philosophy*, aiming at a "revision of the values of the elements of life of individual and community" and trying to attain this aim by

"*a.* establishing and supporting an International Academy for Practical Philosophy and Sociology;

"*b.* establishing and supporting a school for the propagation of the concepts and relations of concepts as introduced by the Academy;

"*c.* other means."

The primordial task of the Academy was intended to be the composing of a new vocabulary on the basis of a linguistic gradation, an item for which we may refer to the first chapter of the present monograph.

A number of thinkers were invited to take part in the activities of the Academy — among them were *Peano*, *Buber*, *Mauthner* and *Tagore* — but soon it became evident that even for the constituent assembly of the Academy financial resources failed.

The only thing left was the continuing of the personal exchange of thought of the founders, which led to the forming of a signific circle in 1922, consisting of *Brouwer*, *Van Eeden*, the linguist *Jac. van Ginneken* (later on professor in the university of Nimeguen) and *G. Mannoury*. This *Signific Circle* proclaimed in their declaration of principles *a. o.* that signific contains more than criticism of language, also more than synthesis of language, and that in opening a deeper insight into the connections between words and the needs and tendencies of the soul, it may affect in a wholesome way the future social and mental conditions of man. But, apart from this, it emphasized the *empirical* tendencies of the signific investigation by adding that this investigation should be carried on more systematically than before and include, besides an introspective investigation into the subconscious elements, the application of experimental and statistical methods.

The *Signific Circle* for some years met regularly, and the discussions held were highly instructive to its members. *Mannoury* kept an accurate account, a small part of which was printed later on (and published in Dutch in the "Synthèse-Series" in 1939). In the meantime, in 1923, there had been issued by the same circle a circular requesting cooperation for a revision of the nomenclature in various branches of science.

After the cessation of the activities of the *Signific Circle* in 1924, the signific methodology was not practised by groups until 1937. During this period *G. Mannoury*, professor of mathematics and of philosophy of mathematics in the University of Amsterdam, also gave courses of lectures on significs, and made a special study of the signific foundations of mathematics. (Cf. his article on "Die signifischen Grundlagen der Mathematik," *Erkenntnis* IV, 1934, the French translation of which was published in the *Collection Synthèse* in 1947). *Van Dantzig*, too, gave lectures on significs in this period.

In 1937 the *International Group for the Study of Significs* was founded on the initiative of the Editorial Board of the journal *Synthèse*. Some of the editors of this journal were attached to the older significists by personal ties of friendship, and through this were familiar with the aims of this movement. Moreover, the program of this journal, aiming at giving expression to divergent systems of intellectual integration, naturally brought the group into contact with the signific views. *Mannoury* himself was one of the founders of the new and renewed circle.

Until the second World War the *International Group for the*

Study of Significs continued its activities on an international scale. It characterized its main objective as

> "The practising of analytic and synthetic significs in general, and its application to the theories of the foundations of the sociological, cultural, political and exact sciences in particular."

The working method of the group was as follows: the signific principles and the method to be applied were discussed in a "central section." In this section were also representatives of the "border-groups," including sections for biology, psychology, general epistemology, ethics and jurisprudence.

During the last year of the German occupation of the Netherlands, where the main seat of the group was located, the work of the *International Group for the Study of Significs* had to be abandoned altogether, but soon after the end of the war it was resumed, though by partly different contributors. The *International Society for Significs* was founded by representatives of both the exact and the social sciences.

The new program pursues theoretical as well as more practical aims; it continues the research into the theories of the foundations of science and aims at the building up of an efficient scientific terminology; furthermore it promotes the empirical study of group languages (content analysis, slogan analysis, bias analysis, etc.).[17]

In 1937 a regular exchange of views and publications was planned with movements characteristic of a critical attitude towards linguistic phenomena, and especially with the *Unity of Science Movement*, with whom we may also reckon the *Warsaw School* (*Lukasiewicz, Kotarbinski, Lesniewski, Tarski, Adjukiewicz, Chwistek*, and some others). In a special section of the journal *Synthèse*, called "Unity of Science Forum," conducted by *Philipp Frank*, *Charles W. Morris* and *Otto Neurath*, leading logical empiricists have explained their views on a renewed criticism of language. Since 1948, *Philipp Frank* and *Charles Morris* have been the editors of a section in *Synthèse* under the heading "Logical Empiricism", in which a series of papers have been and will be published which treat Logical Empiricism as a phenomenon in the intellectual history of the 20th century.

For some years papers on sociological subjects have been prepared for publication in *Synthèse* by the *Institute Français de Sociologie* at Paris, counting among their members sociologists who are taking an active part in the activities of the Significs Society.

---

[17] Part of the activities of the *International Society for Significs* have been described in chapter III of this monograph.

In the Spring 1948 issue of *ETC., a review of General Semantics*, the editors announced the institution of regular correspondence with the *International Society for Significs*, and expressed the hope that this exchange will lead to cooperative editorial and research activities between the *International Society for General Semantics* and the *International Society for Significs*. In the course of these five years fruitful discussions on sign problems have resulted from this cooperation.

Every year the *International Society for Significs* organizes an international conference dealing with problems on criticism of language and the relation between language and behavior. The *Eighth International Signific Summer Conference* was held in September 1952 and was devoted to the study of "Non-scientific elements in the development of science." The proceedings of these conferences and also some of the reports of the sections of the Society are printed in *Synthèse*.

Glossary

This glossary includes only some of the main terms used in the text of the present monograph. It aims to facilitate the reading of this text by giving a definition of those terms which are or might be used in a somewhat other acceptation in other theories of signs. Most of the terms are also defined in the text. The monograph being written in conversational language, the concepts employed are by far not defined in signific terms. In the definitions given below vernacular terms operate as primitive terms.

Act of communication In a wider sense: an act by which one individual or a group of individuals (the "speaker") aims at influencing the behavior of another individual or group of individuals (the "hearer"); in a narrower sense: the linguistic behavior characterized by the use of word-language, either spoken or written or denoted by signals.

Analytic significs See *Significs*.

Concept Psychic correlate of a class of words having a similar function or tendency.

Dispersion Variability of the signification of an act of communication with regard to different individuals or to one individual under emotive or environmental influences. (Intersubjective and intrasubjective dispersion.)

Functional elements Elements of the active and passive signification, to be distinguished in *indicative, emotive* and *volitive* elements, according as they bear upon the representations, the "distribution of affection" and the intention of speaker and hearer.

Hearer See *Act of communication.*

Language In a wider sense: the procedures by which one mind may affect another; in a narrower sense: the vocal and auditive means of communication.

Psychic correlate Mental associations connected with a word-picture or a group of word-pictures.

Signification In a wider sense (the signification of an act of communication): the influence aimed at by the speaker and which the hearer undergoes (active and passive signification), and also the environmental influences affecting the act of communication (symptomatic signification); in a narrower sense (signification of a word): the function of a word or an expression in the act of communication in which this word or this expression is used.

Significs Systematic study of the mental and linguistic phenomena of communication, to be distinguished in *Analytic significs,* being the psychological investigation of the existing means of communication, and *Synthetic significs,* aiming at extending and improving the means of communication in use.

Speaker See *Act of communication.*

Synthetic significs See *Significs.*

Word-image The mental associations connected with the
Word-picture hearing, the reading, the pronouncing or the writing down of a word (auditive, visual, and motorial word-picture).

Bibliography

I. BOOKS AND ARTICLES

The following list of books and articles dealing with psychologically and logically oriented concept analysis and concept synthesis is far from being complete. In this selection the reader will be able to find a more detailed discussion of the fundamental problems referred to in the monograph. The publications that are starred have been the subject of study and discussion in one or more of the sections of the *International Society for Significs.*

Adjukiewicz, Kazimierz. " Sprache und Sinn." *Erkenntnis* IV, 2, 1934.

————. " Logic and Experience." *Synthèse* VIII. 1950-51.

Ayer, A. J. *Language, Truth and Logic.* London : Victor Gollancz, Ltd. : 1946.

*Bentley, Arthur F. *Linguistic Analysis of Mathematics.* Bloomington, Ind. : The Principia Press, 1932.

————. *Behavior, Knowledge, Fact.* Bloomington, Ind. : The Principia Press, 1935.

————. " Kennetic Inquiry." *Science* 112, 2922, pp. 775-783.

————. See Dewey, John.

Beth, Evert W. " De significa van de pasigrafische systemen " (A signific study of the pasigraphic systems). *Euclides* XIII, 1936-1937.

————. *Wijsbegeerte der wiskunde* (A Philosophy of Mathematics). See esp. the chapter on Significs, pp. 304-324. Antwerp : Nimeguen, 1948.

*Bloomfield, Leonard. *Language.* New York : H. Holt and Company, 1933.

————. " Linguistic Aspects of Science." *Philosophy of Science* II, 1935.

*————. " Linguistic Aspects of Science." *International Encyclopedia of Unified Science* I, 4. Univ. of Chicago Press, 1939.

*Bridgman, P. W. *Logic of Modern Physics.* New York : Macmillan, 1938.

*Brouwer, L. E. J. *Over de grondslagen der wiskunde* (Diss.) (On the Foundations of Mathematics). Amsterdam : Maas & Van Suchtelen, 1907.

*————. "Addenda en corrigenda." *Proceedings Koninkl. Akademie van Wetenschappen,* Amsterdam, 1917.

*————. " Wissenschaft, Mathematik und Sprache." *Monatshefte für Mathem. und Physik,* 36-I, 1929.

*————. *Willen, weten, spreken* (Willing, Knowing, Speaking). *De uitdrukkingswijze der wetenschap.* (See Section III of this Bibliography.)

*————. *Signifische dialogen.* (See Section V of this Bibliography.)

*————. " Synopsis of the Signific Movement in the Netherlands." Prospects of the Signific Movement. *Synthèse* V, 1947.

Brunswik, Egon. " The Conceptual Framework of Psychology." *International Encyclopedia of Unified Science,* I, 10. Univ. of Chicago Press, 1952.

Bühler, Karl. *Sprachtheorie.* Jena : G. Fischer, 1934.

Bussy, I. J. de. *Inleiding tot de ethiek* (Introduction to Ethics). Amsterdam : J. H. de Bussy, 1898.

*Bussy, I. J. de. *De wetenschap der moraal* (The Science of Ethics). Amsterdam: J. H. de Bussy, 1939.

*Carnap, Rudolf. *Logische Syntax der Sprache.* Wien: J. Springer, 1934.

*————. *Scheinprobleme in der Philosophie. Das Fremdpsychische und der Realismusstreit.* Berlin: Weltkreis Verlag, 1928.

*————. "Foundations of Logic and Mathematics." *International Encyclopedia of Unified Science* I, 3. Univ. of Chicago Press, 1939.

*————. *Introduction to Semantics.* Harvard University Press, 1942.

Chase, Stuart. *The Tyranny of Words.* New York: Harcourt, Brace, 1938.

Clay, J. *Het begrip natuurwet in de nieuwere wijsbegeerte* (The Notion of Law of Nature in modern philosophy). Leyden: E. J. Brill, 1915.

*————. *Begrip en Begripsvorming* (Concept and Concept Synthesis). *De uitdrukkingswijze der wetenschap.* (See Section III of this Bibliography.)

*————. "The Regression of the Unstructural." *Erkenntnis* VII, 1939.

Daan, Albert. "Strekking en Taak der Significa" (Task and Tendency of Significs). *Synthèse* IV, 3, 1939.

*Dantzig, D. van: "Over de maatschappelijke waarde van onderwijs in wiskunde" (On the Social Value of the Teaching of Mathematics). *Nieuw Tijdschrift voor Wiskunde,* III, 1926.

————. "Some Possibilities of the Future Development of the Notions of Space and Time." *Erkenntnis* VII, 1938.

*————. "Mathematische en empirische grondslagen der waarschijnlijkheidsrekening" (Mathematical and Empirical Foundations of the Theory of Probability). *Ned. Tijdschr. v. Natuurkunde,* VIII, 1941.

————. "General Procedures of Empirical Science." *Synthèse* V, 1947.

*Dewey, John. "Theory of Valuation." *Encyclopedia of Unified Science* II, 4. Univ. of Chicago Press, 1939.

Dewey, John and Bentley, Arthur F. *Knowing and The Known.* Boston: Beacon Press, 1949.

Ducasse, C. J. "Symbols, Signs and Signals." *Journal of Symbolic Logic* IV, 1939.

————. *Philosophy as a Science.* New York: O. Piest, 1941.

————. "Reality, Science and Metaphysics." *Synthèse* VIII, 1 and 2.

*Eeden, Frederik. *Redekunstige grondslag van verstandhouding* (Logical Foundation of Mutual Understanding). Studies III. Amsterdam: Versluys, 1897.

*————. *Signifische Dialogen.* (See Section V of this Bibliography.)

Eisenson, Jon. *The Psychology of Speech.* New York: F. S. Crofts & Co., 1938.

Esch, W. "Taal, Moraal en Signifika als doeltreffende methode van onderzoek" (Language, Ethics and Significs as a useful method of investigation). *Synthèse* IV, 1, 1939.

Esser, P. H. "Waan als meerwaardige term" (Paranoia as a multivalent term). *Psychiatrische en Neurologische Bladen,* 1939, No. 4.

Esser, P. H. "On Word-Fact Relations." *Synthèse* VI, 1947-48.

*Frank, Philipp. *Das Kausalgesetz und seine Grenzen.* Wien: Springer, 1931.

*———. "Foundations of Physics." *International Encyclopedia of Unified Science* I, 7. Univ. of Chicago Press, 1946.

*———. *Modern Science and Its Philosophy.* Harvard Univ. Press, 1949.

———. *Relativity — a Richer Truth.* Boston: The Beacon Press, 1950.

Frenkel-Brunswik, Else. *Motivation and Behavior.* Provincetown, Mass.: The Journal Press, 1942.

*Ginneken S. J., Jac. van. *Principes de Linguistique Psychologique.* (Diss.) Amsterdam: E. v. d. Vecht, 1907.

*———. *De roman van een kleuter* (The Romantic Story of a Little Boy). Nijmegen: Malmberg, 1917.

*———. *Significsche Dialogen.* (See Section V of this Bibliography.)

*Gonseth, Ferdinand. *La logique en tant que physique de l'objet quelconque. Actes du Congrès international de philosophie scientifique.* Paris, 1933.

*———. *Qu'est-ce que la logique? Actualités scientifiques et industrielles.* Paris: Hermann & Cie, 1937.

*Gödel, Kurt. *On undecidable propositions of formal mathematical systems.* Princeton, 1934.

Groenewold, H. J. *Quantum Mechanics and Its Models.* To be published in *Synthèse.*

*Haan, Jacob Israël de. *Rechtskundige significa en hare toepassing op de begrippen: "aansprakelijk, verantwoordelijk, toerekeningsvatbaar"* (Juridical Significs and its application to the notions "liable, responsible, accountable"). (Diss.) Amsterdam: W. Versluys, 1916.

*———. *Rechtskundige significa* (Juridical Significs). Amsterdam: Joh. Müller, 1919.

*Hayakawa, S. I. *Language in Thought and Action.* New York: Harcourt, Brace, 1949.

*Heyting, A. "Die formalen Regeln der intuitionistischen Logik." *Sitzungsber. der Preuss. Akad. d. Wissenschaften,* 1930.

*———. "Die formalen Regeln der intuitionistischen Mathematik." *Sitzungsber. der Preuss. Akad. der Wissenschaften,* 1930.

*———. *Mathematische Grundlagenforschung. Intuitionismus. Beweistheorie.* Berlin: J. Springer, 1934.

*Hijmans, I. H. *Het recht der werkelijkheid* (The realistic conception of law). Haarlem: F. Bohn, 1910.

*Hull, Clark L. *Principles of Behavior.* New York: D. Appleton-Century, 1943.

*Johnson, Wendell. *People in Quandaries: the Semantics of Personal Adjustment.* New York: Harper & Brothers, 1946.

Jordan, H. J. "Causaliteit en causale structuren; de beteekenis van deze begrippen voor de wetenschap van het leven" (Kausalität und kausale Strukturen, die Bedeutung dieser Begriffe für die Wissenschaft vom Leben). *Synthèse* IV, 2, 1939.

Jörgensen, J. *A Treatise of Formal Logic* (3 vols). London, 1931.

Kardiner, Abram. *The Psychological Frontiers of Society.* New York: Columbia Univ. Press, 1945.

Kattsoff, Louis O. *A Philosophy of Mathematics.* (Esp. chap. 13: Mannoury's signific point of view.) Iowa State College Press, 1948.

*Korzybski, A. *Science and Sanity. An Introduction to Non-Aristotelian Systems and General Semantics.* Lancaster, Pa.: The Science Press Printing Company, 2nd Edition, 1941.

Lee, Irving J. *Language Habits in Human Affairs; An Introduction to General Semantics.* New York: Harper & Brothers, 1941.

Lukasiewicz, J. "Philosophische Bemerkungen zu mehrwertigen Systemen des Aussagenkalküls." *C. R. Soc. d. Sciences et des Lettres Varsovie,* Cl. III, 23. 1930.

——— und Tarski, A. "Untersuchungen über den Aussagenkalkül." *C. R. Soc. des Sciences et des Lettres Varsovie,* Cl. III, 23. 1930.

*Mannoury, G. *Methodologisches und Philosophisches zur Elementar-Mathematik.* Haarlem: P. Visser Azn., 1909.

*———. *Over de sociale betekenis van de wiskundige denkvorm* (On the social significance of the mathematical way of thinking). Gröningen: P. Noordhoff, 1917.

*———. *Mathesis en Mystiek* (Mathesis and Mysticism). Amsterdam: Maatschappij voor goede en geedkoope lectuur, 1925.

———. *Les deux pôles de l'esprit.* (French translation by L. Roelandt.) Paris: Librairie du Travail, 1933.

*———. *De signifiese methode van taal- en begrippenonderzoek* (The Signific Method of inquiring into language and concepts). *De uitdrukkingswijze der wetenschap.* (See Section III of this Bibliography.)

*———. "Die signifischen Grundlagen der Mathematik." *Erkenntnis* IV, 1934.

*———. "De 'Wiener Kreis' en de signifische begrippenanalyse" (The "Wiener Kreis" and the signific concept analysis). *Ned. Tijdschrift voor Wijsbegeerte en Psychologie,* 1935.

*———. "Zur Begriffsbestimmung der Signifik." *Synthèse,* 1936.

*———. "Psikologia analizo de la matematika pensmaniero." *Synthèse* II, 1937.

*———. "De schoonheid der wiskunde als signifisch probleem" (The Aesthetic Element of Mathematics as a Signific Problem). *Synthèse* II, 1937.

*———. *Woord en gedachte. Een inleiding tot de signifika, inzonderheid met het oog op het onderwijs in de wiskunde* (Speaking and Thinking. An Introduction to Significs, particularly with a view to the Teaching of Mathematics). Gröningen: P. Noordhoff, 1931.

*———. *Nu en Morgen* (Today and Tomorrow). Signific Varia. Partly published in *Synthèse* IV, 1939.

*———. *Signifik. Einheitswissenschaft.* The Hague: W. P. van Stockum en Zoon, 1938.

*Mannoury, G. "Enige opmerkingen over taalgradatie" (Some remarks on linguistic levels). *Synthèse* III, 1938.

————. "La question vitale 'A ou B.'" *Nieuw Archief voor wiskunde,* 1943.

*————. "Signifische Analyse der Willenssprache als Grundlage einer physikalistischen Sprache." *Erkenntnis* VII, 1938.

*————. *Relativisme en Dialektiek. Schema ener filosofischsociologische grondslagenleer* (Outline of a Theory of the Philosophic Foundations of Sociology). Bussum: F. G. Kroonder, 1948.

*————. *Handboek der Analytische Signifika* (Manual of Analytic Significs. I. Historical Part. II. Fundamental Notions and Methods). Bussum: F. G. Kroonder, 1948.

————. *Polairpsychologische Begripssynthese* (Synthetic Significs). In preparation.

*————. *Signifika* (An Introduction to Significs). The Hague: Servire, 1949.

*————. *Signifische dialogen.* (See Section V of this Bibliography.)

*Mises, Richard von. *Kleines Lehrbuch des Positivismus. Einführung in die empiristische Wissenschaftsauffassung.* The Hague: W. P. van Stockum en Zoon, 1939. (Eng. ed., Harvard Univ. Press, 1951.)

*Morris, Charles. *Six Theories of Mind.* Univ. of Chicago Press, 1932.

*————. *Logical Positivism, Pragmatism, and Scientific Empiricism.* Paris: Hermann et Cie, 1937.

*————. "Foundations of the Theory of Signs." *International Encyclopedia of Unified Science* I, 2. Univ. of Chicago Press, 1938.

————. "Esthetics and the Theory of Signs." *Journal of Unified Science,* 8, 1939.

*————. *Signs, Language, and Behavior.* New York: Prentice-Hall, Inc., 1946.

Naess, Arne. "Ueber die Funktion der Verallgemeinerung." *Erkenntnis* VII, 3, 1938.

*————. *"Truth" as conceived by those who are not professional philosophers.* Oslo, 1938.

*————. "Toward a Theory of Interpretation and Preciseness." *Theoria,* XV, 1949.

*Neurath, Otto. "Protokollsätze." *Erkenntnis.* 1932.

————. *Einheitswissenschaft und Psychologie. Einheitswissenschaft,* Heft I. The Hague: W. P. van Stockum en Zoon, 1938.

*————. "Foundations of the Social Sciences." *International Encyclopedia of Unified Science* II, 1.

*Ogden, C. K., and Richards, I. A. *The Meaning of Meaning.* London: K. Paul, Trench, Trubner & Co., Ltd., 1923.

Peters, J. M. L. *De taal van de film* (The Language of the Film). The Hague: A. N. Govers N. V., 1951.

*Piaget, Jean. *Le développement de la notion de temps chez l'enfant.* Presses Universitaires de France, 1945.

*Piaget, Jean. *Les notions de mouvement et de vitesse chez l'enfant.* Presses Universitaires de France, 1946.

*———— et Inhelder, B. *La géométrie spontanée de l'enfant.* Presses Universitaires de France, 1949.

*————. " Du rapport des sciences avec la philosophie." *Synthèse* VI, 3 and 4, 1947-1948.

*————. " L'analyse psycho-génétique et l'épistémologie des sciences exactes." *Synthèse* VII, 1 and 2, 1948-1949.

*————. *Traité de Logique.* Paris: Librairie Armand Colin, 1949.

*————. *Introduction à l'épistémologie génétique.* 3 vols. Presses Universitaires de France, 1949.

*Pos, H. J. *De taal als symbolische functie* (Language as a symbolic function). *De uitdrukkingswijze der wetenschap.* (See Section III of this Bibliography.)

*Rapoport, Anatol. " Mathematical Biophysics, Cybernetics and Significs." *Synthèse* VIII, 3 and 5, 1950-1951.

————. *Science and the Goals of Man, a Study in Semantic Orientation.* New York : Harper & Brothers, 1950.

*Reichenbach, Hans. *Experience and Prediction.* Univ. of Chicago Press, 1938.

*————. *Philosophic Foundations of Quantum Mechanics.* Berkeley : Univ. of California Press, 1944.

*————. *Elements of Symbolic Logic.* New York: Macmillan, 1948.

*Russell, Bertrand. *The Principles of Mathematics.* Vol. I. London-New York, 1938.

*Schächter, Josef. *Prolegomena zu einer kritischen Grammatik.* Wien: Julius Springer, 1935.

*————. " Der Sinn pessimistischer Sätze." *Synthèse* III, 1938.

*Schlick, Moritz. *Allgemeine Erkenntnislehre.* Berlin: Springer, 1928.

*————. *Fragen der Ethik.* Wien, 1930.

*————. *Gesammelte Aufsätze.* Wien: Gerold & Co., 1938.

Servien, Pius. *Le langage des sciences.* Paris: Librairie scientifique Albert Blanchard, 1931.

*Shannon, Claude E. and Weaver, Warren. *The Mathematical Theory of Communication.* Univ. of Illinois Press, 1949.

Stebbing, L. Susan. *Thinking to some Purpose.* Harmondsworth-New York: Penguin Books, 1938.

Stutterheim, C. F. P. " Signifique et Linguistique." *Synthèse* VI, 1947.

Tönnessen, Herman. *Det private Initiativ, En semantisk-sosiologisk undersökelse* (Private enterprise, a semantic study). Oslo: Universitetets Studentkontor, 1948.

Vuysje, D. " Significa." *Synthèse* I, 1936.

————. " Ernst Mach en onze tijd " (Ernst Mach and the present epoch). *Synthèse* IV, 1939.

Vuysje, D. " Begripsvorming en Taal " (Concept Synthesis and Language). *Synthèse* IV, 1939.

————. " Der Terminus ' Dialektik ' im politischen Sprachgebrauch, eine signifische Untersuchung." *Bibliothèque Scientifique* 15, pp. 133-141. *Dialectica*, II, 1948.

————. *Pour une meilleure intelligence mutuelle. Humanime économique*, Sept.-Oct. 1938. Paris: Groupe d'Etudes de l'Humanisme Economique.

————. *Psycho-linguistique et Logique.* XVIe Congrès International de Philosophie des Sciences. Colloque de Logique. Paris: Hermann & Cie, 1949.

*Waismann, Friedrich. *Einführung in das mathematische Denken.* Wien: Gerold & Co., 1936.

*————. *Vorwort* (in Moritz Schlick: Gesammelte Aufsätze). Wien: Gerold & Co., 1938.

*————. " Von der Natur eines philosophischen Problems." *Synthèse* IV, 1939.

*————. " The many-level structure of language." *Synthèse* V, 1946.

Walter, Emil J. " Die Bedeutung der Tiefenpsychologie für die geschichtliche und soziologische Forschung." *Synthèse* VI, 1947-1948.

————. *Psychologische Grundlagen der geschichtlichen und sozialen Entwicklung.* Zurich: Pan-Verlag, 1947.

*Welby, Lady Victoria. *Significs and Language.* London: Macmillan, 1911.

*Westendorp Boerma, N. *Realistische Ethiek* (Realistic Ethics). Amsterdam: H. J. Paris, 1935.

————. *De wetenschapsopvatting van een theoloog* (The conception of science of a theologian). Amsterdam: H. J. Paris, 1939.

Witte O. P., A. J. J. de. " De Signifische Beweging " (The Signific Movement). *Kultuurleven* XVIII, 2, 1951.

*Wittgenstein, Ludwig. *Tractatus logico-philosophicus.* New York: Harcourt, Brace, 1922.

*Woodger, J. H. *The Axiomatic Method in Biology.* Cambridge, 1937.

————. " The Technique of Theory Construction." *International Encylopedia of Unified Science.* Univ. of Chicago Press, 1939.

II. REPORTS

Reports on the discussions on epistemological, logical, mathematical, biological, psychological and sociological subjects held in the various sections of the *Society.* These mimeographed reports are for the greater part written in Dutch, but some of them have been published in English in the journal *Synthèse.*

Published in separate form: *Provisional Report on an Inquiry into the Usage of Some Political Terms in the Netherlands*, 1948. This inquiry was instituted on the request of Unesco.

III. COLLECTIONS OF ARTICLES AND DISCUSSIONS

1. *Communications of the International Institute for Philosophy at Amsterdam*, I, March 1918. II, March 1919. Gröningen: P. Noordhoff. Members of the Managing Committee of the Institute were: G. Mannoury, president; L. E. J. Brouwer, secretary; Henri Borel, Frederik van Eeden.
2. *De uitdrukkingswijze der wetenschap* (Ways of Expression of Science). A series of lectures on epistemological subjects held at the University of Amsterdam during the academic year 1932-1933 by L. E. J. Brouwer, J. Clay, A. H. de Hartog, G. Mannoury, H. J. Pos, G. Révész, J. Tinbergen and J. van der Waals, Jr. Gröningen: P. Noordhoff. 1933.
3. *Significsche dialogen*. (See Section V of this Bibliography.)
4. *Proceedings of the Fourth International Significal Summer Conference. Synthèse*, VII, 1-2. 1948-1949. Bussum: F. G. Kroonder.
5. *Proceedings of the Fifth International Significal Summer Conference. Synthèse*, VIII, 3-5. 1950-1951. Bussum: F. G. Kroonder.

IV. SYNTHESE

Synthèse, An international journal for the logical and psychological Study of the Foundations of Science, comprising Communications of the *Institute for the Unity of Science, International Society for Significs, Institut Français de Sociologie* and of the French and Dutch Sections of the *International Association for Logic and the Philosophy of Science.* General Editorial Committee: J. Clay (Amsterdam), P. H. Esser (Bennebroek), Philipp Frank (Cambridge, Mass.), J. C. L. Godefroy (Amsterdam), B. H. Kazemier (The Hague), W. M. Kruseman (Deventer), Arne Naess (Oslo), Chr. P. Raven (Utrecht), D. Vuysje (Amsterdam), N. Westendorp Boerma (Amsterdam), J. H. Woodger (London). Publisher: F. G. Kroonder, Bussum (Netherlands).

V. SYNTHESE SERIES

A series of books on logical, (psycho-) linguistic and psychological subjects.

1. *Significsche dialogen* (Signific Dialogues). By L. E. J. Brouwer, Fred. van Eeden, Jac. van Ginneken S. J. and G. Mannoury. Utrecht: J. Bijleveld, 1939.
2. Bochenski, O. P., I. M.: *Précis de Logique Mathématique.* Bussum: F. G. Kroonder, 1948.
3. Mannoury, G.: *Les Fondements psycho-linguistiques des Mathématiques.* Bussum: F. G. Kroonder, Neuchâtel: Editions du Griffon, 1947.

Proceedings of the American Academy of Arts and Sciences

Vol. 80, No. 4, P. 271-350, March, 1954

CONTRIBUTIONS TO THE ANALYSIS AND SYNTHESIS OF KNOWLEDGE

4

Published in cooperation with the
Institute for the unity of Science

PSYCHOANALYSIS AND THE UNITY OF SCIENCE

Else Frenkel-Brunswik
University of California

CONTENTS

PAGE

1. The Broadening of Operationism 273
2. The Concept of the Unconscious 277
3. The Concept of Instinct 286
4. Remoteness from the Phenotype 292
5. The Explanatory Value of Psychoanalysis. Motivation and Behavior 300
6. Are Psychoanalytic Concepts Hypothetical Constructs or Intervening Variables? 307
7. Attempts at Confirmation of Psychoanalytic Hypotheses 314
8. Formal Criteria in a Study of Self-Deception . . . 320
9. Psychodynamics and Cognition 322
10. Psychoanalysis, Ethics, and Rationality 331

Summary 337

References 343

Prepared for the *Institute for the Unity of Science* in Boston, for publication in the *Proceedings of the American Academy of Arts and Sciences.*

Psychoanalysis and the Unity of Science [*]

ELSE FRENKEL-BRUNSWIK
University of California

1. THE BROADENING OF OPERATIONISM

Freud's first formulations of his ideas in the sphere of personality aroused a clamor of protest which has never entirely subsided. More recently, however, we may observe a shift in the type of criticism levelled against psychoanalysis. Once the initial shock ensuing from Freud's discoveries had been overcome, scientists were in a better position to look at the structure of the theory of personality proposed by psychoanalysis, and the more recent scrutinies of the system appear to be concerned to a greater extent with its formal or methodological characteristics than with its content. Thus we hear that the method employed by psychoanalysis is a subjective rather than an objective one; that hypotheses and facts are often confused; that the psychoanalytic propositions do not lend themselves to scientific verification; that concepts like superego, ego and id are foisted on us as entities in a manner characteristic of the pre-scientific stages of inquiry and formulation; that many of the attempts to verify psychoanalytic assumptions have actually failed.

---

[*] Some of the contentions of this paper were first presented at the Symposium on the Experimental Approach to Psychoanalytic Theory held at the Meeting of the American Psychological Association in 1950, and also at seminar meetings in the Universities of California, Chicago and Michigan in 1951 and 1952. Some of the theoretical parts of the paper in its present version were read at the Berkeley Conference for the Unity of Science, July 1953, and at the San Francisco Psychoanalytic Society.

The philosophical arguments brought forth in this paper derive from some of the teachings of logical positivism. The writer is indebted primarily to her teacher in the philosophy of science, Moritz Schlick, in whose seminars and discussion groups she participated during and after her studies at the University of Vienna. In effect, this paper constitutes a return to an assignment, suggested to the writer some twenty years ago by the late Otto Neurath, to clarify the standing of psychoanalysis in the framework of the Unity of Science movement inaugurated by him at that time.

The common theme of these charges seems to be a dissatisfaction concerning the relationship between concept and fact, or between theory and observation, in psychoanalytic theory. We are not suggesting that all of these strictures are unjustified. Before examining concept formation as it occurs in psychoanalysis and existing attempts at confirmation of psychoanalytic hypotheses, we should, however, first like to recall, in a more general way, some of the more recent teachings concerning the relation between theory and observation. We should like to show that some – though by no means all – of the criticisms raised against psychoanalysis are based on misconceptions about this relationship. Many of the objections against psychoanalysis have their origin in an overly narrow interpretation of the version of scientific empiricism commonly known as operationism with its stress on the rooting of all concepts in concrete manipulations and observations (*11*, for a summary of psychological applications see *67*), and generally in a vaguely anti-theoretical attitude, rather than in a legitimate criticism of psychoanalytic theory. This holds especially for the criticism originating in psychology proper.

As operationism is an offshoot of some considerations in physics, many critics of psychoanalysis explicitly point to the physical sciences as providing an ideal model for the formulation of theory, psychoanalytic or otherwise. However, this is in most cases done without full realization of the extent of certain changes in the conception of theoretical structure which have taken place in the field of physics itself. These developments in physics made it necessary to modify the requirements stated in the older forms of empiricism and positivism. Thus Einstein (*20*) does not demand that all abstract terms of science should be interpreted in terms of sense observation, and he designates as " structural " those elements of a theory which deal only with the relationship between the symbols.

> In order that thinking might not degenerate into metaphysics or into empty talk, it is only necessary that enough propositions of the conceptual system be firmly enough connected with sensory experiences.

It was Schlick (*60*) who as early as 1920 stressed the departure from the phenomenally given as one of the foremost accomplishments of Einstein's theory. Schlick emphasized the importance of both " observational " and " auxiliary " concepts such as electric forces. Since the latter are also measurable, he was at that time even inclined to consider them to be as " real " as colors and tones. He changed his position later, however. At the present time we witness a return to Schlick's critical realism (*61*) within the movement of logical positivism, notably by Feigl (*23*). Instead of the phenotypical approach we now acknowledge what Philipp Frank (*26*) has labeled the non-pictorial type of theorizing. The inability of our imagination to follow the notions of a four-dimensional space is only one of the most trivial examples of this. Frank points out that the earlier ultra-positivistic requirement, according to which all principles of physics should be formulated by using only observable quantities, has been replaced by a weaker requirement; now it is merely postulated that from these principles mathematical conclusions could be drawn that would be connected by semantic rules with statements about observable facts. Einstein speaks of

> the ever-widening logical gap between basic concepts and laws, on the one hand, and the consequences to be correlated with our experiences, on the other.

Along similar lines move Hempel's (*44*) formulations concerning the necessary revisions of operationism and empiricism:

> But it is precisely those fictitious concepts rather than those fully definable by observables which enable science to interpret and organize the data of direct observation by means of a coherent system which permits explanation and prediction.

There is increasing realization of the fact that scientific theories are the product of our imagination even though the system must have a rooting in observable fact and experience remains the ultimate and sole criterion of its adequacy. Different levels of description and theory can be defined by the closeness of the concept to, or their remoteness from, observable facts. Hempel gives a vivid description of the relationship between a scientific theory and the observational data.

> The whole system floats, as it were, above the plane of observation and is anchored to it by rules of interpre-

> tation. These might be viewed as strings which are not part of the network but link certain points of the latter with specific places in the plane of observation. By virtue of those interpretive connections, the network can function as a scientific theory: from certain observational data, we may ascend, via an interpretive string, to some point in the theoretical network, thence proceed, via definitions and hypotheses, to other points from which another interpretive string permits a descent to the plane of observation.

A comparison between the developments in physics and the situation in psychoanalysis is certainly not in all respects justified. While we must grant to the psychoanalytic system many more elements of a truly scientific theory than most philosophers of science realize, it still remains true that psychoanalysis contains many metaphors, analogies, and confusions between constructs and facts of which it can and must in the end be rid.

However, some parallels between recent developments in physics and psychoanalysis can be pointed out, and these have implications upon the problem as to what are the requirements for a confirmation of psychoanalytic propositions. Modern physics and psychoanalysis have in common a turning away from the " natural " to a " fictitious " language. And the common result of this policy is that a wider and simpler network of interrelationships within observable data is ultimately being achieved. Freud has introduced a number of fictitious constructs, such as unconsciousness, id, superego, repression, death instinct, and so forth. Although we may grant that psychoanalysis, in trying to avoid the error of an overly descriptive, segmentary and specific approach, has sometimes fallen into the error of over-interpretation and of drawing far-reaching conclusions from fragmentary evidence, we still contend that at one point the jump into theory which Hempel advocates must be made. The fact that theoretical constructs of the kind mentioned above refer only indirectly, and not completely at that, to observable data must therefore not be made the basis of an objection against psychoanalysis as such. It would constitute a serious misunderstanding of scientific theory construction were we to go along with the all too frequent type of criticism of fictitious constructs recently exemplified by Ellis (*21*) who, in referring to such concepts as id and superego, writes as follows:

> Many professional workers will immediately object that while they have frequently come across sex-desires, guilt-feelings and conflicts in their patients, they have never had the pleasure of greeting an ego or superego.

At the same time we must stress that the links to the observational data must be specified. Freud started with rather specific observations concerning hysteria and repression of the sexual content in hysteria. While at the beginning of his psychoanalytic explorations Freud kept close to the data and introduced constructs which he needed more directly for description and explanation of his data, he later became more speculative. From these speculations he derived a number of theorems which he then tested in his clinical observations. Then he became speculative once more to an extent which may be considered controversial, as we shall see in our discussion of the death instinct.

2. The Concept of the Unconscious

There are two aspects of psychoanalytic theory which must be held apart. The first is given by its formal aspects, that is, the way the constructs are defined and the relationship between the various concepts. The second consists in the problems of verification or confirmation of psychoanalytic statements. We will begin with a discussion of the way Freud himself introduced some of his crucial concepts and will concentrate on two of the most basic and at the same time most controversial, that of the unconscious and that of instinct. Later we will turn to an analysis of these concepts from the point of view of recent notions on scientific concept formation.

As far as the definition of basic concepts is concerned, many critics of psychoanalysis have objected to an alleged lack of sophistication in Freud concerning the philosophy of science, and to his tendency to " reify " his concepts. The quotations from Freud that follow are brought forth as evidence to the contrary. They are to demonstrate how keenly aware Freud was of logical and epistemological problems, a fact often forgotten under the impact of the carelessness with which some of his followers use his terms.

About the function and nature of definitions in science, Freud has the following to say:

> The view is often defended that sciences should be built up on clear and sharply defined basal concepts. In actual fact no science, not even the most exact, begins with such definitions. The true beginning of scientific activity consists rather in describing phenomena and then in proceeding to group, classify and correlate them. Even at the stage of description it is not possible to avoid applying certain abstract ideas to the material in hand, ideas derived from various sources and certainly not the fruit of the new experience only. Still more indispensable are such ideas – which will later become the basal concepts of the science – as the material is further elaborated. They must at first necessarily possess some measure of uncertainty; there can be no question of any clear delimitation of their content. So long as they remain in this condition, we come to an understanding about their meaning by repeated references to the material of observation. . . . Strictly speaking, they are in the nature of conventions; although everything depends on their being chosen in no arbitrary manner, but determined by the important relations they have to the empirical material – relations that we seem to divine before we can clearly recognize and demonstrate them. It is only after more searching investigation of the field in question that we are able to formulate with increased clarity the scientific concepts underlying it, and progressively so to modify these concepts that they become widely applicable and at the same time consistent logically. Then, indeed, it may be time to immure them in definitions. The progress of science, however, demands a certain elasticity even in these definitions. The science of physics furnishes an excellent illustration of the way in which even those ‘basal concepts’ that are firmly established in the form of definitions are constantly being altered in their content (*34*, p. 60f).

Freud further stresses the fact that we also make use of certain “complicated postulates” to guide us in dealing with psychological phenomena. Freud is aware of the fact that our observations are guided by explicit or implicit theoretical assumptions. Thus most clinical descriptions found in Freud employ the inferential construct of the unconscious. Freud proceeds from the common observation that an idea which is now present in consciousness may become absent in the next moment, and may become present again, after an interval, unchanged and from memory rather than as a result of a fresh sensory perception. It is this fact which leads Freud to the " supposition " that during the interval the conception has been present, although in a " latent " stage. In this he follows such famous precedent as that of Plato or Herbart. Freud argues that an unconscious conception is one of which we are not aware, but " the existence of which we are nevertheless ready to admit on account of other proofs or signs." (*33*, p. 22).

Among other things it is post-hypnotic suggestion which is brought forth as evidence for the importance of the distinction between conscious and unconscious. According to Freud no account of the post-hypnotic execution of an order, received during hypnosis and of which there is no recollection, can be given unless we assume a condition of latency, or of unconscious presence, of the command. Freud further points out that not all of the idea re-emerges into consciousness, but only the conception of the act to be executed.

From the standpoint of modern logic of science, there is nothing objectionable about the notion of latent or unconscious tendencies, at least not so long as we do not insist on assigning them to " the mind " in a metaphysical sense. A comparable situation obtains in physics when we observe that a physical object at one time exhibits magnetism — that is, when a piece of iron is present in its vicinity — and that it does not do so when this condition is not fulfilled. Magnetism thus is what Carnap (*15*) has called a " dispositional concept." The precise logical nature of concepts of this kind in relation to psychoanalysis will be taken up later. Suffice it to say here that such composite terms as unconscious hostility or dependency describe a disposition to display aggression or dependence under specified conditions, for example in therapy. We may remember in this context the definition of behavior given by Carnap at another occasion (*16*) which includes the just-mentioned processes with which psychoanalysis deals:

> It must be made clear that the term behavior has a greater extension here than it tended to have with the early behaviorists. Here it is intended to designate not only overt behavior which can be observed externally but also internal behavior, that is, processes within the organism; in addition, it includes dispositions to behavior which may not be manifest in a given special case; and finally it includes certain effects of overt behavior upon the environment.

In addition to the post-hypnotic execution of hypnotic suggestions as a means of confirming unconscious factors as determinants of behavior, the transition of latent processes into manifest behavior may also be observed more directly in the psychoanalytic procedure. Freud considers the assumption of unconsciousness as necessary because the data of consciousness are " exceedingly defective." Conscious acts alone do not enable us to account for the parapraxes and dreams of healthy persons and of the mental symptoms or obsessions in the sick. Our most intimate daily experience introduces us " to sudden ideas of the source of which we are ignorant, and to results of mentation arrived at we know not how " (*35*, p. 99). All these conscious acts are said to remain disconnected and unintelligible if we are determined to hold fast to the claim that every single mental act performed within us must be consciously experienced. On the other hand, it is pointed out, they fall into a demonstrable connection if we interpolate the unconscious acts that we infer. The gain in meaning and connection is considered by Freud a perfectly justifiable motive, one which may well " carry us beyond the limitations of direct experience."

In the opinion of Freud it is the very assumption of unconscious processes which enables psychoanalysis to take its place as " a natural science like any other." He goes on to explain that these processes are " in themselves just as unknowable as those dealt with by other sciences such as physics and chemistry " (*41*). And Freud remains in the spirit of the natural sciences when he states that

> it is possible to establish the laws which those processes obey and to follow over long and unbroken stretches their mutual relation and interdependences (*41*, p. 36).

The introduction of the term " unconsciousness " thus is done in the service of good scientific procedure and with explicit reference to its rules. The relationships between consciousness, preconsciousness and unconsciousness are described in detail by Freud, and he designates this field as the topographical aspect of psychoanalysis. Latent ideas which are capable of entering consciousness without any special resistance are called preconscious, while the term unconscious is reserved for ideas " with a certain dynamic character " which are kept " apart from consciousness in spite of their intensity and activity."

The concepts of conscious and unconscious thus emerge as signification of "particular systems and possessed of certain characteristics." Freud was keenly aware of the fact that he was building a formal model containing the different systems with a specification of the relationships between them. The following quotation shows how far he was from a reification of his concepts:

> We might still attempt to avoid confusion by employing for the recognized mental systems certain arbitrarily chosen names which have no reference to consciousness. Only we should first have to justify the principles on which we distinguish the systems and we should not be able to ignore the question of consciousness, seeing that it forms the point of departure for all our investigations. Perhaps we may look for some assistance from the proposal to employ, at any rate in writing, the abbreviation Cs for consciousness and the Ucs for the unconscious when we are using the two words in the systematic sense (*35*, p. 105).

From then on Freud consistently used these symbols, as if to underscore the fact that they refer to mere abstractions. Freud further points to the fact that by the introduction of the different mental systems he has departed from the descriptive academic psychology and that the consideration of " mental topography," which means the indication " in respect of any given mental operation within what system or between what systems it runs its course," has won psychoanalysis the name of depth psychology.

Freud thus introduced the concept of the unconscious as an abstract, hypothetical construct (see the discussion later) which is specified by its relationship to other concepts such as consciousness and preconsciousness and to which a partial empirical interpretation is given by reference to free associations, dreams, and occurrences during therapy. In order to establish the relationships to observational data, many subsidiary hypotheses such as those concerning the nature of the therapeutic process and of dreams are necessary. In his later writings, notably in *New Introductory Lectures* (*39*) and in *An Outline of Psychoanalysis* (*41*, p. 33f), Freud prefers to use the term " unconscious " as denoting mental qualities rather than systems, at the same time replacing it by the more obviously dispositional term " Id " for the major purposes of the theory.

> We will no longer use the word unconscious in the sense of a system and to what we have hitherto called by that name we will give a better one which will not give rise to misunderstanding. We will call it henceforth the Id.

These and related formulations of Freud are in our opinion less lucid than the ones discussed before. The reason for Freud's proposal to shift from the interpretation of the unconscious as a " system " to that as a " mental " quality lies in his later discovery that not only the " Id " but also part of the " Superego " and " Ego " are often unconscious. In our opinion this discovery does not necessarily interfere with an interpretation of the unconscious as a system with specific dispositional characteristics. Incidentally, it may be helpful in the early stages of discovery to designate certain patterns of behavior in terms of special and relatively fixed classifications such as id, ego, and superego. Today many of the earlier statements of psychoanalysis may be reformulated in terms of behavioral patterns in such a manner that the facets of behavior connected with the biological processes are differentiated from those which are the result of cultural taboos.

There is no reason to assume that Freud's writings necessarily improved as time progressed. It is interesting to speculate as to whether Freud first felt the need of thinking of the unconscious as an abstract system, and then his increasing familiarity with the manifestations which were to be explained by the term once chosen made him view these attributes as mental qualities. In his paper on "existential hypotheses," Feigl (*23*) has pointed out that at the beginning of a science it is often difficult to judge which concepts represent mere conventions and which refer to specific entities. Although Freud has never explicitly designated the concept of unconscious as a mere convention, in his above quoted interpretation of the unconscious as a system to be designated merely by letters or letter-combinations it seems to entail more conventionalist connotations than in his later writings. In view of Freud's original intent in introducing the concept of the unconscious, such criticism as the following by Ellis (*21*) seems unjustified:

> It is now (thanks to Freud) a well established clinical fact that human beings have thoughts and feelings of which they are unaware or unconscious. But to collect the processes under the term unconscious, used as noun and implying a specific entity in itself, is more confusing than revealing.

In discussing the localization of mental processes, Freud points out that the objection may be raised that these latent recollections can no longer be described as mental processes but that they correspond to residues of somatic processes. Against this he holds that the conventional identification of the mental with the conscious is thoroughly unpractical since it forces us "prematurely to retire from the territory of psychological research without being able to offer us any compensation elsewhere." He concludes that we shall therefore be better advised to give prominence to "what we know with certainty of the nature of these debatable states" (*35*, p. 99ff).

Freud goes on to acknowledge that a "rough correlation of . . . the mental apparatus to anatomy . . . exists." But in his opinion every attempt to deduce from these facts a localization of mental processes has completely miscarried. He points to the "hiatus which at present cannot be filled, nor is it one of the tasks of psychology to fill it." Our mental topography, he concludes, has for the present nothing to do with anatomy; it is concerned not with anatomical locations, but with "regions in the mental apparatus" irrespective of their possible situation in the body. The fact that, at least for the time being, a physiological interpretation of his concepts has not become possible is stressed by Freud again and again. While at the beginning Freud was intensely dominated by neurophysiological thinking, the decisive progress in psychoanalysis occurred not until after he freed himself from the search for such analogies and turned to more openly psychological models. However, Freud always considered this step as temporary and as necessitated by the present imperfect state of the biological sciences.

We cannot do more than give a few examples of the manysidedness of Freud's models. Thus Freud goes into great detail in discussing the fate of ideas and affects in terms of a " given volume of excitation," developing the point of view which he has called " economic." In an earlier paper Freud characterizes this energy as " something having all the attributes of a quantity although we possess no means of measuring it." He adopts Helmholtz's principle of conservation of energy. Siegfried Bernfeld (*5*) has amply pointed out Helmholtz's influence on Freud. The energy withdrawn from a conscious idea when it is repressed is used for an anti-cathexis of the same idea, which makes for keeping the idea out of consciousness. Freud uses the various formal relations between the different systems to give an explanation for the subvarieties of repression occurring in hysteria, phobia, and other forms of neurosis. In the discussion of an animal phobia which constitutes a substitute for the hate of the father, Freud (*35*) compares the real fear of an animal with the neurotic one. The latter is " fed . . . from the springs of unconscious instinct " and is " obdurate and extravagant in the face of all influences brought to bear from the system Cs, . . . thereby betraying its origin in the system Ucs."

There are said to be several differentiating characteristics between the Ucs system and Cs system. The conscious ideas comprise the " concrete idea " plus the " verbal idea " corresponding to it. By linking the two, a higher form of organization can be achieved. The processes of the Ucs, on the other hand, are little related to reality. They are subject to the pleasure principle, and the external reality is substituted by an internal reality. There is exemption from mutual contradiction and the outcome depends mainly upon the degree of strength of these processes and upon the " degree of their regulation by pleasure and pain." Or, as Freud says later, we deal here with mobile and unbound energies. The processes of this system are " not ordered temporally," are not altered by the passage of time. All these characteristics are grouped together under the heading of " primary processes." These are genetically succeeded by the " secondary processes " which occur within the Cs system. This latter system binds the primitive energies through reality-control.

The derivatives of the Ucs act especially through dreams and free association as intermediaries between the two systems. Closer study of the derivatives of the Ucs will altogether disappoint our expectations of a schematically clear division of the one mental system from the other. A good illustration of inferences from the " manifest " to the " latent " content is given by the following quotation on dream interpretations:

> The dreamer's associations bring to light intermediate links which we can then insert in the gap between the two and with the help of which we can recover the latent material of the dream and " interpret " it. It is not to be wondered at that this work of interpretation (acting in a direction opposite to that of the dream-work) fails occasionally to find a completely certain conclusion (*41*, p. 54).

In defending all these complexities Freud stresses the aim of merely translating into theory the results of observation. He points out that there is no obligation to achieve at our very first attempt a theory that " commends itself by its simplicity, in which all is plain sailing." Freud argues that we must defend complexities of the theory so long as we find that they fit in with the results of observation; and we must not abandon our expectation of being guided in the end by those very complexities to recognition of " a state of affairs that is at once simple in itself and at the same time answers to all the complications of reality " (*35*, p. 122f). There is an obvious similarity between these and many of Freud's earlier quoted programmatic utterances, and those of logical empiricists; this adherence extends here to the postulate of simplicity and becomes even more pointed by the relegation of simple structure to a distant ideal state of affairs.

3. The Concept of Instinct

Next to the concept of the unconscious it is that of instinct which has been objected to most vigorously in the face of claims of psychoanalysis for consideration as a science. According to Freud, the concept of instinct is a " conventional but still rather obscure " one. Considering mental life from a biological point of view, an instinct appears to Freud as both the " mental representative " of the stimuli emanating from within the organism and as a " measure of the demand made upon the energy of the latter in consequence of its connection with the body " (*34*, pp. 61, 64). Statements concerning animal drives like hunger or sex would not seem to require any special justification so long as they remain mere descriptive summaries of factual behavior. However, the further elaboration of the concept of instinct as representing a special kind of energy and a constancy principle for this energy, as well as the more specifically psychoanalytic doctrine of the far-reaching transformations and disguises, particularly of the sex instinct, renders the concept of instinct a much more involved one, leading Freud to the following simile:

> The theory of the instincts is, as it were, our mythology. The instincts are mythical beings, superb in their indefiniteness. In our work we cannot for a moment overlook them, and yet we are never certain that we are seeing them clearly (*39*, p. 131).

There are several ways in which this statement may be interpreted. Freud may have had in mind that the concept of instinct is hypothetical in spite of the fact that we are aware of some of the instinctual impulses. In this case the term " mythology " may be misleading. What Freud probably had in mind over and beyond the interpretation just given is the lack of knowledge about the basic biological processes which underlie the instincts, and consequently a certain lack of clarity in his own instinct theory. Although we must grant that this is true, we must add that the sections dealing with infantile sexuality and with the description of the psychosexual stages of development belong to the most lucid and most powerfully executed parts of Freud's system.

After differentiating and defining the " impetus," " object," " source," and " aim " of instincts, Freud undertakes to classify the instincts, pointing out that there is " obviously a great opportunity here for arbitrary choice." In this enterprise we should not neglect to ask whether instinctual motives do not admit of further analysis in respect to their sources, so that only those primal instincts " which are not to be resolved further " could really lay claim to the name (*34*, p. 67).

Pursuing the psychological description, Freud summarizes the distinctive characteristics of instincts by pointing out that instincts have in a high degree the capacity to " act vicariously for one another " and that they can readily change their objects. Thus they are capable of activities widely removed from their original modes of attaining their aims, such as in " sublimation " (p. 69).

The very fact that the concept of instinct covers a wide variety of behavior and permits of many manifestations accounts for its explanatory value. Only if Freud would have ascribed to every variety of manifest behavior a corresponding instinct would he have rendered this concept superfluous and circular. Freud nonetheless expresses some doubt whether the psychological approach alone will afford any decisive indication for the distinction and classification of instincts. He himself has wavered in his classification of instincts. At first he differentiated the sex- from the ego-instincts (self-preservation). The ego-instincts include further all the counter-sexual forces such as guilt feelings, ethical ideals, etc. However, later, Freud viewed " egoism " or " narcissism " as of the same libidinal character as sex. He therefore gave up his original classification of instincts, and later he distinguished Eros and Death, in which process aggression acquired a primary status not further deducible.

The " death instinct " is Freud's perhaps most controversial speculation. It assumes that there is a basic tendency in living organisms as well as in inorganic systems towards self-destruction, reduction of tension, and Nirvana; in contrast to this, Eros represents initiative and hunger for stimulation. Even such an ardent follower of Freud as Fenichel (*24*) tends to the belief that the death instinct is a superfluous and far-fetched concept even in the explanation of such facts as depression, masochism and guilt-feelings. According to Fenichel, " external factors that disturbed the principles innate to the organism " could be adduced instead of a separate " genuine self-destructive instinct." However, definite judgment on this matter should be postponed. Freud himself has stressed that much will depend on results of future biological research. Freud's own tentativeness concerning these assumptions is strikingly expressed in the following passages, showing, at the same time, once more Freud's astuteness in matters of the philosophy of science:

> One may surely give oneself up to a line of thought, and following it up as far as it leads, simply out of scientific curiosity, or – if you prefer – as *advocatus diaboli*, without, however, making a pact with the devil about it. . . . At all events, there is no way of working out this idea except by combining facts with pure imagination many times in succession and thereby departing far from observation. We know that the final result becomes the more untrustworthy the oftener one does this in the course of building up a theory, but the precise degree of uncertainty is not ascertainable. One may have gone ignominiously astray (*37*, p. 190f).

A large part of psychoanalytic theory deals with the vicissitudes of the instincts and their transformations, such as reversal into the opposite, repression, and sublimation. These concepts do not pose the same logical questions as, for instance, the concept of the unconscious; they refer to interrelationships among facts and successions of patterns, all of which are more or less directly observable. But since these transformations include a very long span of time and permit of alternative manifestations, we soon get involved in problems of the methods of verification and confirmation; these we shall take up in later sections. Here we should like to give just an example of how Freud arrives at the assumption that such traits as exceptional orderliness, cleanliness, parsimony, and obstinacy, in adults, are related to anality and the process of bowel movement in early childhood, thus constituting aspects of the instinctual pattern of anal eroticism. Freud first observed the repeated coexistence of the traits mentioned and then turned to the history of the individuals who exhibited them. These were found to have been in the habit of refusing to empty the bowel, deriving an incidental pleasure from the act of defecation. Since these peculiarities tend to disappear once childhood has passed, the anal zone seems to have lost its erotogenic significance; but the constant appearance of the traits listed may, according to Freud, be brought into relation with the disappearance of the anal erotism of the individuals concerned. Freud proceeds to show the theoretical relation between the trait-syndrome and activities related to the anal zone. The triad of cleanliness, orderliness, and reliability are said to give exactly the impression of a reaction-formation against an interest in things that are unclean and intrusive and ought not to be on the body. Freud refers to the saying, " Dirt is matter in the wrong place." He admits that to bring obstinacy into relation with interest in defecation seems no easy task, but points to the fact that infants can very early behave with great self-will about parting with their stools (*32*, p. 45). Freud introduces materials from ancient civilizations, myths, fairy tales, dreams and superstitions as a further means of verification of the anal theory by demonstrating that in these archaic modes of thought money comes into a close relationship with excrement.

The correlation between the three traits of cleanliness, orderliness, and obstinacy has been statistically confirmed by Sears (*63*). In the psychoanalytic system these empirical relations are not disconnected from such theoretical considerations as those on infantile sexuality and repression. Freud's scientific procedure can be illustrated once more by following him in his theoretical deduction of the observation that one may expect but little of the " anal character " in adults who have not repressed the erogenic quality of the anal zone, as for example certain homosexuals.

In addition to the evidence from genetic sequences and from symbolic materials like dreams and myths, Freud attempts to support the transformation of the original interest in excrement and dirt into a later interest in cleanliness and orderliness by referring to the occasional breaking through of the older attitude in spite of the fact that a reaction formation took place, and to the exaggeration of the new attitude which from the point of view of " economy " seems to indicate a struggle against a force in the opposite direction. An independent empirical study which has led me to a systematic inventory of criteria of this kind will be discussed below.

While Freud gave prominence to such physical activities as oral and anal ones and considered them as prototypes and models for other types of activities, the Neo-Freudians consider these bodily activities as but one manifestation of broader social events. Only a large and presently unavailable body of data could help us choose between these alternative hypotheses. On the basis of the evidence accumulated thus far it would seem that although bodily events have a distinction of their own, they still are capable of being, and actually are, modified by the larger cultural contexts in which they obviously are embedded.

The material discussed in this and the preceding section seems to warrant the conclusion that Freud was aware of the principal problems of theory construction and of the philosophy of science. On the whole he introduces his concepts very carefully, distinguishing what we now call the postulatory from the operational elements of the theory and allowing their interplay as he moves along. However, some more formal attempts in the direction of an axiomatization of the psychoanalytic system would be quite useful. We are not speaking here of a logical or quantitative formalization in the strict sense but merely of the more systematic differentiation between basic assumptions and their derivations. For example, a combination of the assumptions of infantile sexuality and of repression may be able to cover many of the more specific theorems in psychoanalysis. As we have seen, Freud himself has derived the concept of unconsciousness from his observations about repression. Such an attempt at a systematization of psychoanalysis would be helpful in uncovering the logical contradictions, empirical gaps, insufficient evidences, and related flaws inherent in the system. In spite of its suggestive character and in the main logically appropriate approach, psychoanalysis still remains in many aspects programmatic, as Freud himself has so often emphasized.

It must furthermore not be forgotten that Freud often talks in an abbreviated manner, for example when he speaks about the " existence of the unconscious " or when he uses such expressions as " the super-ego, or the ego " being such and such, or doing this and that. However, this manner of speech does not, as a rule, lead to serious consequences. Freud always continued to work on the improvement of his concepts and their relations to each other, and to check them against observational data. The two procedures are often combined in his writings, but mostly it is not difficult to discern which definitions are " syntactical " and which " semantical."

In spite of Freud's caution and his fine sense for scientific procedure, psychoanalytic theory remains vague in many places. Freud is aware of this fact; in one place he speaks of the " superb indefiniteness " of the term " instinct." But he tries to defend concepts of this kind when he says:

> We can claim for them the same value as approximations as belongs to the corresponding intellectual scaffolding found in other natural sciences and we look forward to their being modified, corrected and more precisely determined as more experience is accumulated and sifted. So too it will be entirely in accordance with our expectations if the basic concepts and principles of the new science (instinct, nervous energy, etc.) remain for a considerable time no less indeterminate than those of the older sciences (force, mass, attraction, etc.) (*41*, p. 36).

In this context we may recall a statement made by Otto Neurath to the effect that " new ideas of scientific importance start mostly with vague and sometimes queer explanations," and we should heed his advice that " one can love exactness and nevertheless consciously tolerate a certain amount of vagueness " (*55*, p. 21), an advice which he apparently considers necessary to give to an empiricist.

Pointing to the frequent use of analogies, many critics of Freud feel, and not entirely without justification, that Freud at times exceeded the legitimate degree of vagueness. Freud himself thinks that some of the difficulties in his speculations concerning the life and death instincts come from our being obliged to operate with " metaphorical expressions peculiar to psychology (or more correctly psychology of the deeper layers)."

Freud assumes that some of the shortcomings would disappear if for the psychological terms we could substitute " physiological or chemical " ones. Freud explicitly states that until then we have to deal with analogies, and that analogies " prove nothing." Their function is to make one " feel more at home " (*39*, p. 103).

We come here to perhaps the most serious objection which can be raised against the psychoanalytic system. This is given by the occasional use of analogies by Freud which seem to go distinctly beyond the heuristic stage. In discussing the unconscious, Freud himself raises the question whether or not he is just dealing with animistic analogies. He points out that the psychoanalytic assumption of unconscious mental activity may be nothing but a further development of that primitive animism which caused our own consciousness to be reflected in all around us (*35*, p. 104). After some further deliberation, however, Freud rejects his own objection, and we must agree with him as far as the particular case in question is concerned.

There also are many mechanistic analogies in Freud, although these are mostly used for the explicit purpose of stating mere formal similarities. One becomes impressed by the carefulness with which Freud avoids an over-simplified and uncritical dualistic psychophysical parallelism. He speaks of the " insoluble difficulties " of psychophysical parallelism, or of interactionism for that matter, and often prefers to talk about the psychological vs. the physiological " language," or about physiology as slated some day to supersede psychology, rather than about causal relationships between two metaphysically distinct systems.

While the analogical procedure is not suited for purposes of ultimately proving a scientific hypothesis, it must be stressed that the function of analogy is more important in psychology than it is in physics. In physics pictorial types of relationships are available as an important heuristic guide prior to the establishment of abstractly and objectively defined terms used in theory construction. In the early stages of scientific theory construction, such as we are witnessing in psychoanalysis, in which the abstract terms are not yet fully defined, the analogy plays an important role in the discovery and communication of relationships. Their use by Freud turned out to be extremely fruitful, perhaps because it was coupled with a sense of responsibility for scientific procedure which led to his being aware most of the time of the stage of scientific inquiry and testing in which he happened to be involved.

4. Remoteness from the Phenotype

Freud's turning away from the directly observable, obvious, face-value picture of personality and from its common-sense interpretation which is illustrated in his use of the concept of unconscious and of instinct is one of the most impressive and at the same time one of the most bewildering aspects of psychoanalytic theory.

This departure from the obvious, directly observed phenomenon which we shall now study in its relational aspects has taken two different forms. First, there is an attention for certain relationships which had remained largely unexplored before the advent of psychoanalysis. Observations of pathological states prompted Freud to explore infantile sexuality, dreams, the psychopathology of everyday life, the seemingly irrational types of symptoms, both normal and neurotic, and eventually fairy tales, myths, and works of art. All these unusual types of data were exposed by him to scientific inquiry. The second surprise element in Freud's approach lies in the unusual and unfamiliar interpretations which were given to familiar data of overt behavior. In either case certain forms of behavior were examined in a wider genetic and dynamic context and inferences drawn which went beyond — and often were in seeming contradiction to — the obvious gross characteristics of behavior. Sometimes these gross characteristics are called " phenotypical " in psychology, in a modified usage of the familiar biological term. The inferences concerned are in line with the departure from the immediate pictorial appearance in modern physics to which we have referred in the introductory section.

To many critics it has seemed disturbing that in certain psychoanalytic contexts extreme friendliness is interpreted as a sign of hostility, extreme tidiness as a sign of preoccupation with dirt, or extreme boredom as a sign of intense interest. However, this seeming discrepancy with observation does not mean that there is no set of specified conditions and rules which determine when friendliness is to be interpreted as friendliness and when as aggression. We will discuss those specifications in a moment. But we must anticipate there that it often turns out that if we draw our inferences from a greater variety of manifestations, using maximal as well as minimal cues, instead of taking verbalized statements or social techniques as directly valid, we arrive at a more fruitful and unified interpretation of personality which furnishes an improved basis for long-range predictions. We may go so far as to say that it is the very shift from the level of external, overt manifestation to the level of motivational dynamics which opens the way to a science of personality. This shift is altogether the merit of psychoanalysis.

In some previous discussion of psychoanalysis the present writer

(*28*) has suggested that it is primarily the interpretative extrapolation into a hypothetical central region that justifies the use of the label, depth psychology, as contrasted with the more conventional surface psychology which concentrates upon the directly accessible gross features of behavior or of experienced phenomena at their face value.

Scientific inference concerning central processes, that is, the assumption of internal states on the basis of external evidence, cannot be defended unless it is based on a wide variety of circumstantial evidence. In this sense central inference had never been truly attempted before psychoanalysis. Correspondingly psychoanalysis, especially in its beginnings, comparatively neglected not only the surface observations in their specific identity but also the so-called " distal achievements " – that is, end results – of behavior (*28*). As was pointed out by Egon Brunswik (*13*), the major objective criteria for units of action in psychology, such as those suggested by Albert P. Weiss, Hunter, Tolman, and Hull, are explicitly and implicitly based on a recognition of " vicarious functioning " of behavioral means relative to the same distal goal.

The approach of psychoanalysis shows important differences from this approach to behavior, both falling short of it and going beyond it. The regrouping of manifest observable facts was at least in the beginnings of psychoanalysis undertaken exclusively in terms of " cause " or " need " rather than in terms of " effect " (as would be the case in Tolman's purposive behaviorism, *68*). Not only are there alternative manifestations to the same need in psychoanalysis, there even are alternative goals to the same need. The chief differentiating characteristic of psychoanalysis is to have pointed out sameness of cause rather than sameness of end. Thus the conscious or unconscious aspects of our reactions are not in the same way alternative as, for instance, running or swimming toward the same goal, to cite one of the experimental examples of Tolman. One may argue that if somebody gives up the goal of, say, playing with dirt, for, say, painting, still the same goal is perceived. Against this objection it could be pointed out that such sublimated activities as painting represent a scattering to essentially shifted forms of adjustment and behavioral effects in spite of their asserted relatedness, in a genetic manner, to the same primitive, instinctual tendencies.

In the discussion of Freudian theory, the influence of Darwin is usually stressed. Freud himself has made some remarks to this effect in his autobiography (*40*). This influence is certainly discernible as far as his theory of evolution is concerned. However, unlike that of Darwin, Freud's view and classification of behavior phenomena is not based on criteria of " adjustment."

In discussing the influence of Darwin on Freud, Bergmann (*3*) stresses as a positive feature the naturalistic approach and as negative what he calls the teleological aspects of the Freudian doctrine, that is, explanations from the point of view of a goal, such as survival. It will be remembered that we have taken the opposite stand, that is, that psychoanalytic explanations are oriented primarily toward causes rather than toward ends.

The relative absence of distal result reference, that is, of emphasis on fixed goals and environmental effects in Freud's theory which the writer has pointed out from the standpoint of academic psychology (*28*), has also been noted by some of Freud's most devoted intimates. Using somewhat different means of conceptualization, Anna Freud (*31*) has pointed out that at least in the more orthodox forms of psychoanalytic theory and diagnosis all efforts concentrate backward into " depth," that is, toward the common " historical " origin of the various manifestations. More recently, Suzanne Bernfeld (*6*) has related this predilection of Freud for " digging " into the past of the individual to his fondness for archaeology which she has shown to have been his only major hobby.

In defense of the depth-psychological type of approach it must be emphasized that the temporary abandonment of concern with the reaching of goals has turned out to be an eye-opener for all the vastly different manifestations which may be linked functionally to an assumed common drive. One of the most fruitful consequences of this seems to me to be that it now becomes possible to link the motivational patterns thus uncovered not only to gross features of behavior but also to minimal cues. It is crucial that neither the gross nor the subtle features are taken " at face value " in this process. One of the main advantages to be gained from the concept of drives would be lost if we would fail to transcend the straight description or direct categorization of behavior. Quite aside from the validity of the psychoanalytic inferences, one is forced to recognize that they have shown a great resolving power in bringing together conceptually the most diverse varieties of apparently unrelated behavioral features.

The exclusive emphasis on manifest behavior and its further effects proved to be sterile both in the study of personality and in the earlier, static classificatory systems in psychiatry. It is to a large extent the fact that in our culture basic impulses must be disguised or transformed to pass individual and social censorship which makes it necessary to unearth the hidden themes of motivation. Academic psychology dealt chiefly with types of behavior in which there is more continuity all through the course of life, and was less concerned with the kind of behavior which gets drastically deflected and changes the basic form of manifestation as development proceeds.

In the majority of cases it is verbal behavior such as dreams, free associations, and the like, rather than overt motor behavior which psychoanalysis takes as the basis for drive interpretations. This does not mean that psychoanalysis is " introspectionistic," however. As everyone knows, it is precisely through psychoanalysis that we have learned to doubt the face value of introspection. In behaviorism we often find the tacit assumption that motor behavior is superior to verbal behavior as a validating criterion of constructs. This predilection for overt behavior as the final measure of validity is not justified. We can " lie " with gestures as well as we can with words, either deliberately or as a matter of unrecognized practice. If we observe the motor behavior of two individuals in their interaction we may be more misled as to their basic attitudes toward each other than if we study the verbal statements they make about each other.

However, the shift of emphasis from overt behavior to underlying dynamics was too radical in psychoanalysis. Even though it remains true that the variety of isolated behavioral manifestations is too diverse for scientific penetration, integration in terms of unorganized, instinctual dynamics alone is too universal an integration. While academic psychology and sociology tended to abstract only the gross features of behavior whenever they tried to cope with the unmanageable variety of behavioral manifestations, psychoanalysis did not altogether avoid the pitfall of motivational relativism and a genetic dissolution of overt adjustmental values.

Most of Freud's thoughts are concerned with the innate instinctual development, while the discussion of the modification of the instincts under the restrictions and disapproval of the environment takes a secondary place. Such widely different patterns of social adjustment as that of, say, a criminal and a lawyer, a " saint " and a sexual masochist, are not adequately differentiated from one another in terms of the psychoanalytic theory of instinct taken in and by itself. Although it is of utmost importance to become cognizant of what these socially so diametrically opposite patterns may have in common, the differences existing between them must also be accounted for in any full-fledged and behaviorally oriented theory of personality. The concept of " sublimation " adduced in psychoanalysis to explain the transformation of certain instinctual energies into socially accepted channels and goals remains vague and the conditions of this mechanism appear not to be fully specified in psychoanalytic theory. This shortcoming has been noted by some orthodox psychoanalytic theorists also.

In fairness to Freud it must be stressed that he has made a definite effort to move from his predominant orientation toward the unconscious, irrational and " archaic " to a consideration of so-called reality factors in his psychology of the ego and of the defense mechanisms. Some of the differentiations just mentioned are introduced there. However, Freud has subordinated the development of the ego with its reality-oriented behavior under the supremacy of the id factors, that is, under the internal, biologically determined instinctual impulses:

> It is easy to see that the ego is that part of the id which has been modified by the direct influence of the external world through the perception-consciousness.

Freud tends to view character structure from a defensive point of view, and social influences as a series of traumata which bring to a halt or discontinue instinctual gratification and expression. While providing an understanding of an important aspect of the individual's attitude to society, this view does not do justice to all the satisfactions gained from moving along constructive social avenues.

Within psychoanalysis, it was Heinz Hartmann (*43*) who first stressed explicitly the partial independence of such outwardly adjustive processes as perception, learning and thinking from the instinctual processes. In general, psychoanalysis has undergone, during the last few decades, some modification in the direction of including just those problems which it previously neglected. However, psychoanalytic expansion in this direction has been more programmatic than real, and there are a number of problems which can be solved only by an explicit integration of psychoanalysis with psychology and with sociology. The conceptual tools of psychoanalysis just are not sufficient fully to explain rational and social behavior (*28*). If we were to deny this we were to obscure the essential theoretical contribution of Freud. Some of the resistances against admitting psychoanalysis to the circle of respectable sciences may well be based on an unrecognized transfer from the realization of the failure of psychoanalysis to deal adequately with reasoning behavior.

While the neglect of certain environmental factors in Freud's theory can hardly be questioned, there are, at the other extreme, such overt behavioral psychologists as Skinner (*66*) who tend to look askance at the assumption of special internal dynamisms and attempt to offer an explanation of the facts observed by Freud in terms of direct relationships between resultant behavior and the stimulus environment. However, a translation of the psychoanalytic concepts into the terminology of such a stimulus-response approach, useful as it may be in certain contexts, has its difficulties and limitations. The specific contribution psychoanalysis may be able to make to stimulus-response psychology by stressing the function of internal agencies is best illustrated by means of the superego concept. Although the superego designates that aspect of our behavior dispositions which is most markedly determined by the transmission – via stimulus-response learning – of cultural norms and ethical standards, psychoanalytic theory at the same time stresses the irrational distortions which the external authorities undergo in the process of their absorption into the superego. Thus arises the seemingly paradoxical picture of a superego which originates outside – being an introjection of parental authorities and of the norms they represent – and which then becomes endowed with all kinds of unsocialized subjective tendencies (such as aggression) so that the end effect is frequently an irrational and unconscious distortion of the very values it is supposed to represent. Thus our moral feelings can probably not be fully explained by external cultural factors; they must be seen as reinforced by the internal, hostile feelings with which, in our childhood, we projectively imbued parental figures.

The very fact that moral behavior and especially moral feelings cannot be derived exclusively from the cultural institutions as learned by the example of adults makes it understandable that patterns of behavior acquired in childhood are often intensified in old age by means of some processes of internalization in spite of the fact that they are not reinforced in later life and should therefore, according to the accepted principles of learning theory, slowly succumb to extinction. In 1942 the late Professor Hull, the foremost formalizer of classical learning theory, expressed to me his puzzlement about these facts and the difficulty he had in explaining the superego by his learning theory. Recent learning theories have paid some attention to these problems without, however, giving a fully satisfactory resolution (*65*). Skinner's above-mentioned reservations against psychoanalysis, on the other hand, seem to stem not so much from an involvement with classical learning theory as from a more general anti-theoretical position which is directed against both psychoanalysis and learning theory.

It is interesting to note that quite generally Freud's major contribution to the understanding of social phenomena does not lie in the writings in which he deals directly with sociological phenomena. Here as elsewhere it is his personality theory which, in spite of its limitations, seems to emerge as the most powerful influence in the end. Thus, while such sociologists as Durkheim (*19*) stress conformity to the social institution as something unequivocally positive, psychoanalysis helps us to distinguish the genuine from the compensatory conformity. At the behavioral level, the compensatory type of conformity is characterized by a compulsive, all-or-none character. It is excessive because it compensates for feelings of uncertainty and the attendant fear of becoming an outcast, and because it often serves the function of covering up the underlying resentment toward the social system as a whole, unconscious as this resentment may be. The compulsive conformist tends to adhere to the letter rather than the spirit of the social institutions. This tendency issues from a distortion and simplification of the system of norms and commands in the direction of what one may call unidimensional interpretation. Rules are adopted or enforced which are largely non-functional caricatures of our social institutions, based as they are on a misunderstanding of the ultimate intent of these institutions. The absence of a genuine incorporation of the values of society accounts for the rigidity of the conformity; at the same time it accounts for a certain unreliability, a readiness to shift allegiances suddenly and completely to other, sometimes diametrically opposite authorities or standards. In this and in most other ways the compensatory conformist defeats the very purposes which are inherent in any genuine adherence to a principle. The potential of psychoanalysis eventually to become involved in the systematic treatment of behavioral results after all, is shown to best advantage in the uncovering of fake mechanisms such as that just discussed.

As we have seen, the major emphasis of psychoanalysis is on internal causes; these include, in the language of psychoanalysis, "subjective phantasies" and generally the differential meanings external events acquire for the individual. Freud began to make progress in his understanding of hysteria only after he had given up the idea of a simple environmental causation of the disease by the traumata to which the patients themselves referred. Freud points out that only after the factor of the hysterical phantasies had been introduced did the structure of the neurosis and its relation to the patient "become conspicuous." Since the relationship of these phantasies to external factors is most complex and ambiguous, it seems in the long run far more parsimonious to assume the internal mechanisms postulated by psychoanalysis and to try to specify them operationally as fully as it is possible at a later time. Contrary to Skinner we believe that such assumptions do not carry us outside of the "bounds of natural science." On the other hand, we do agree with Skinner on the point that the "looking inside the organism for an explanation of behavior" can easily lead to a neglect of environmental factors, and we readily acknowledge that it has done so in the case of psychoanalysis.

5. THE EXPLANATORY VALUE OF PSYCHOANALYSIS. MOTIVATION AND BEHAVIOR

In spite of the onesidedness of the depth-psychological approach, the unique contribution of psychoanalysis lies in the unified explanation of personality. Diverse classes of behavioral manifestations are subsumed under more general principles. Behavior is no longer defined in terms of its symptomatic characteristics but is viewed from a causal-genetic point of view.

The tangled relationships between overt behavior and the patterns of underlying "dynamics" have posed numerous methodological difficulties to objectification. There have been two major types of inadequacy in approaching these difficulties. First, some investigators worked on the basis of a tacit assumption of a perfect one-to-one correspondence between motivation and action. The concept of motivation thus is rendered a merely descriptive one; in fact, it becomes nothing but an unnecessary duplication of behavior rather than a truly explanatory, inferential construct imbued with some degree of independence. The scientist or diagnostician must avoid such direct projection of behavioral trends back into the subject without due consideration of the ambiguities inherent in the relationships between drives and behavior. To indulge in such projection in an almost postulatory fashion as is sometimes practiced would expose us to the accusation of circularity. A second and somewhat obsolete type of spurious attempt at explanation is given by the introduction of dynamic concepts which are so remote from behavior that they cannot be confirmed either directly or indirectly.

To help resolve the methodological problem involved in the definition and measurement of motivation separately from, and in a certain sense independently of, isolated bits of specific behavior, and at the same time to illustrate the way in which motivational tendencies can be operationally based on certain intercombinations of observed behaviors, the present writer (*29*) has undertaken a statistical demonstration of the explanatory and predictive character of psychoanalytic concepts based on the systematic study of a larger number of adolescents extending over a period of several years. For a limited set of problems, this was to be an empirical verification of psychoanalysis and was to take the place of the case-centered and thus necessarily somewhat fragmentary and often casual inferences of motivation customary in psychoanalysis. A direct intuitive assessment of underlying motivation was made, and these drive ratings were compared with the specific and directly observable behavioral techniques used by the individuals. Considering the fact that verbalized self-reports and short-sample behavioral techniques often reveal the phenotypical façade of a person rather than the genuinely dynamic tendencies of his behavior, several clinicians, closely familiar with the group of adolescents in question over a number of years, were asked to rate them with respect to the strength of their basic motivation as abstracted from a synopsis of a wide variety of long-range behavior contacts. By introducing motivational ratings we hoped to be able to account for apparently diverse manifestations, and hoped that, just as in psychoanalytic theory, an important lead for uncovering relationships might lie in comparatively few but fundamental characteristics of the subjects brought out in motivational ratings. Different classes of behavioral expressions seemingly not related among each other were found to be related to the same intuitively inferred drive, apparently as alternate manifestations of that drive. To give an example: overt behavioral ratings on " exuberance " and " irritability " were found to intercorrelate negatively with one another ($r = -.52$) and thus to be relatively incompatible; however, both these behavioral traits showed some positive correlation ($+ .30$ and $+ .42$) with the ratings on the drive for aggression. The so-called multiple correlation between rated aggression and the two diverse behavior features was found to be relatively high ($+ .73$), thus establishing the principle of "alternative manifestations" of a drive. Our results can be more generally stated by saying that a knowledge of the type of drive variables stressed in psychoanalysis seems to hold good promise for behavior prediction of the " either-or " type just described statistically, which is in essence the either-criminal-or-lawyer type of prediction of which we have spoken in our above example from the discussion of psychoanalysis.

The results also indicate that we could not arrive at the underlying motivational variables by such conventionally used statistical means as factor analysis, at least not in its various forms customary to date. These and related techniques provide us with clusters of behavioral manifestations which intercorrelate highly with each other. The fact that certain manifestations may fully substitute for others renders the relationship in psychoanalysis that we are looking for much too complex to be handled in this simple manner. The type of evaluation of underlying motivations considered here allows for alternative manifestations of the same motivation or instinct which are negatively correlated or which may even be mutually exclusive even though probably genetically related.

The linking of the motivational ratings to behavioral manifestations constitutes at the same time a " rational reconstruction " of the so-called " intuitive " inferences made by the clinicians. This is done by the statistical establishment of the cues which they as a group used for their ratings. The relatively satisfactory inter-rater agreements which were obtained for the motivational ratings are a further indication of the fact that we are capable not only to perceive the surface manifestations in their own right, but also the underlying personality structure. We " see " not only whether or not a person shows a friendly front but also whether his friendliness is genuine or " phony."

In the present context we are primarily interested in establishing those aspects of behavior that can be predicted on the basis of motivation ratings or a psychoanalytic appraisal of the underlying instinctual structure and those that cannot be predicted on such a basis. Since results suggest that the same underlying motivation may lead to a wide range of behavior, the further specification must hinge on other than " dynamic " factors. Among these further factors determining whether, say, underlying aggression is worked out in a socially constructive form or in neurotic symptoms, such extraneous factors as social and economic or occupational conditions will probably play a major role.

In summary, the study of motivation and behavior just reported constitutes an attempt to move from the level of the description of overt behavior and expressional techniques to the level of inferential hypotheses along basically psychoanalytic lines by discounting incidental effects of transient situations and by regrouping the forms of behavior in terms of common dynamics. In this manner we do arrive at comparatively few underlying tendencies which account for a wide array of behavioral manifestations seemingly unconnected with each other. It may be added that further evidence shows that we also arrive at predictions about phantasies of our subjects which are markedly superior to predictions based on the direct use of overt behavior. In our empirical work our assumption of the increased economy and greater explanatory value of constructs relating to underlying dynamics has been justified by means of a multiple-correlational analysis.

The one major way in which academic psychology has attempted explanations of behavior is by means of physiological models. So far not too many predictions from such models have been deduced, however; and those that have been refer to rather simple types of behavior, e.g., the laws of so-called *Gestalt* perception or some of the basic mechanisms of learning. It is hardly an exaggeration to say that we may disregard these types of explanation as far as the field of personality is concerned. The non-physiologically oriented learning theories have introduced, in somewhat piecemeal fashion, a number of constructs originally restricted to relatively simple types of behavior. There have been many attempts to expand learning theory to the problem area covered by psychoanalysis. As we have exemplified above by means of the example of the progressive non-reinforced internalization of the superego, it seems doubtful that the commonly employed principles of learning are capable to explain all of the major discoveries of psychoanalysis, the efforts of Gustav Bergmann (*3*), of Dollard and Miller (*18*), and of others notwithstanding. I shall come back to this point later.

The view that psychoanalysis is explanatory in contrast to most of academic psychology which moves at the level of description may be supported by reference to Feigl's theoretical discussion of levels of explanation. Feigl (*22*) distinguishes four levels of scientific discourse. The first is the level of description, dealing with specific statements, logically in singular form, by means of which is stated what is directly observed. The second level is that of empirical laws, that is, of generalized functional relationships between relatively directly observable magnitudes. The third level, called first-order theories, involves a set of assumptions from which empirical laws can be derived. It is only by means of theory that a unification and an understanding of the functional relationships between facts as expressed in empirical laws can be achieved. While in first-order theory the constructs used are homogeneous with the operationally defined concepts used in the empirical laws, the constructs of the theories of the second order – the fourth level in Feigl's hierarchy – are heterogeneous to the empirical regularities which the theory purports to explain; an example is the physiological explanation of regularities of overt behavior. Feigl places psychoanalysis at the third level in this scheme, that is, at the level of first-order theories, together with the theories of Tolman and Hull. Although we follow Feigl to this point, the question remains whether or not a certain group of psychoanalytic concepts such as that of the unconscious should be considered to be as heterogeneous to behavior as are physiological constructs or explanations. It should be noted in this context that Feigl's somewhat restrained preference for physiological explanations of behavior is evident in all his writings. It is the assumption of the unconscious which establishes the major difference between psychoanalytic theory construction, on the one hand, and Tolman and Hull, on the other.

Before continuing the discussion of the properties of psychoanalytic concepts we should like to deal shortly with some more vaguely philosophical arguments which question the explanatory value of psychoanalysis. Toulmin (*69*) argues that the psychoanalytic explanations constitute an account of " motives rather than of causes," since they provide the patient with plausible reasons for his neurosis and do not necessarily relate to objective facts. Expanding this argument, Flew (*25*) states that Freud as a working psychoanalyst makes patients realize what their unconscious motives, purposes and intentions are, and then proceeds as a theoretician to assume that the inferred unconscious processes are capable of producing real symptoms while only " real " physiological and neurological processes could do such a thing and mere inferred unconscious processes have nothing to do with causes. Arguments of this kind stem from confusions about the nature of concepts; here we may follow Dingle (*17*), who in his answer to Toulmin and Flew takes the point that the concept of the unconscious is in no way different from that of the atom or of light.

Far from identifying the reported motive and the objective explanation of behavior, the major merit of psychoanalysis is to have differentiated the two and to have unmasked and " discredited " as to their explanatory value the subjective experiences of motivation. The phenotypical, " manifest " characteristics are now taken to provide only the indirect cues for inferences concerning the " latent " genotypical (conditional-genetic) forces of motivation.

In all the efforts to distinguish manifest behavior and latent motivation, surface and depth, our path is full of obstacles, some of which go beyond the merely methodological ones encountered in the procedures of scientific verification. One of these additional difficulties is a semantic one. The vocabulary of everyday language does not furnish us consistently with two separate sets of terms, one for overt behavior and the other for underlying motivation. Unless we are ready drastically to depart from familiar usage of terms, we must use " friendliness " for both the basically friendly outlook on life and for the techniques of friendliness – genuine or fake – by which this basic outlook may be implemented or pretended. The same holds for such terms as " aggression," " submissiveness," etc. Qualifying adjectives are needed to specify the meaning of the word " friendly " or " aggressive " in each case it is used. In the first place, such double usages make for unmanageable clumsiness; second, and more important, is the fact that they present a challenge to our claim that the splitting up of the vocabulary is really necessary.

This dilemma is in a formal sense similar to the one presented by the two-faced meaning of our common perceptual terms which was pointed out by Egon Brunswik (*12*). These terms too tend to have double reference, one to the personal and somewhat variable perceptual response, and the other to the interpersonal measured physical stimulus that underlies the so-called " corresponding " response with some but by no means perfect regularity. Examples are phenomenal versus measured size, shape, color, pitch, and so forth. Phenomenal size or shape can be experimentally shown to depend in a multiple fashion on a variety of measured sizes or shapes in our surroundings or at the retina. The same holds for color, and subjective pitch has since the time of Helmholtz been known to depend on both the frequency and the intensity of sound, so that ' pitch ' is no longer admissible as a synonym or translation for ' frequency.' If in view of these tangled relationships the conceptual separation of perceptual stimulus and response can be said no longer to constitute a case of entities superfluously multiplied; neither can, we may add, the separation of behavior and motivation with their similarly tangled relationships. Each time separations of this kind had to be substituted for previous identifications in the history of science, there was irrational, emotional resistance against the recognition of the equivocations or ambiguities involved. These resistances remind the present writer of the syndrome of " intolerance of ambiguity " she has found to be a major characteristic of the authoritarian personality (*30*).

Certainly, both our "motivations" and "behaviors" are constituted from overt behavior, as both stimuli and perceptual responses constitute different types of observational experiences. But motivations are arrived at through a synopsis of the constant elements in many bits of behavior, while the latter are momentary manifestations. The important point is that the two sets are to some extent operationally independent, and that they turn out to be actually independent to a surprising degree by the frequency of alternative manifestations of identical motivations, and vice versa. An independent nomenclature for the levels will in the end have to be established. This would remove much of the temptation to fall back into an oversimplified single-level treatment of the motivational aspects of behavior as is still present in such recently suggested concepts as that of "manifest need." For example, it can be shown (*28*) that Murray's frequently quoted list of "needs" (*54*) is by no means free of duplications of overt behavior patterns, his own warnings to the contrary notwithstanding.

In the process of the conceptual differentiation between motivation and behavior a further semantic aspect will have to play an integral part. We mean the use of minimal cues such as those to be described later in this paper as an indication of the frankness or "genuineness" of behavioral expressions.

The problem of genuineness of behavior which has proved so crucial for the considerations in this section is comparable to that of strict "correspondence" versus "non-correspondence" between a certain stimulus variable and a certain response variable in perception to which we have referred above, and thus to the problem of the accuracy or correctness of perceptual performance.

6. ARE PSYCHOANALYTIC CONCEPTS HYPOTHETICAL CONSTRUCTS OR INTERVENING VARIABLES?

In a paper which has attracted much attention in the literature on psychological theory, MacCorquodale and Meehl (*50*) proposed to distinguish between "intervening variables" as quantities obtained by specified manipulations of the values of empirically observed variables, and "hypothetical constructs" which involve assumptions about an additional entity, process or event not directly observed. Statements dealing with intervening variables contain no terms which are not definable either directly or by reduction sentences in terms of empirical variables. According to MacCorquodale and Meehl, they represent nothing more than shorthand summaries about observables and contain no inferences as to the occurrence of unobserved events. Carnap's dispositional concepts are given as an illustration of intervening variables, while " electron " is given as an example of a hypothetical construct. While it is admitted that the statements about electrons also are supported by observations, these are said not to exhaust the entire meaning of the sentences about electrons.

In the field of psychology, hypothetical constructs are illustrated by MacCorquodale and Meehl by physioneurological models of behavior. A parallel is drawn between intervening variables and " abstracta " in the sense of Reichenbach (57), and hypothetical constructs as corresponding to Reichenbach's " illata." To the latter inferred entities belong such constructs as other persons' " minds." These are inferred on the basis of our sensory impressions, but statements involving them are not reducible to sentences about impressions; they have what Reichenbach calls a " surplus meaning." MacCorquodale and Meehl assume surplus meaning only in case an effort at explanation from another domain is made. Thus physiological explanations of behavior are assumed to have surplus meaning in the behavioral frame of reference only but not in the framework of physiology proper.

In the rather sketchy application of their distinction to psychoanalysis, MacCorquodale and Meehl point out that psychoanalysts originally introduced such terms as libido, censorship, or superego as intervening variables, claiming them to be conventionalized designations of observable properties; but they add that in the course of further discussion there usually is an unnoticed shift in the direction of using them as hypothetical constructs. The authors point to certain elaborations of the concept of libido which Freud first introduced as an abbreviation for a set of sexual needs but then proceeded to describe as " flowing," as being " dammed up," as " tending to regress to early channels," as " being converted " into anxiety, and as " making its energy available " to the ego. The authors do not object to the concept of libido on the grounds that it refers to unobservables, but merely because the postulates concerning existential properties are not explicitly announced. A further argument which MacCorquodale and Meehl raise against the " hydraulic " analogy of the libido as used by Freud is that analogies of this kind would have to consider the knowledge now available in physiology. They continue to argue that no known properties of nervous tissues could correspond to these hydraulic properties and that the nervous system does not contain tubes and pipes.

Although we find the distinction between intervening variables and hypothetical constructs useful because of its emphasis on differences in the degree of indirectness of evidence, we at the same time consider it in many ways misleading. First, the authors seem to conceive of hypothetical constructs mainly as assumptions about physiological processes. They do not discuss what seems another frequent type of hypothetical construct, that is, one in which the actual properties are neither presently determined nor expected to be determined in the foreseeable future.

Second, by reiterating that intervening variables are definable in terms of observables, the authors fail to stress that statements containing intervening variables can, at least in principle, no more be exhausted by statements about observables than can those about hypothetical constructs. We recall here the changes in the views on verification which occurred within the school of logical positivism. To quote Carnap (*15*):

> If by verification is meant a definite and final establishment of truth, then no synthetical sentence is ever verifiable. We can only confirm a sentence more and more. Therefore we shall speak of the problem of confirmation rather than of verification.

According to Carnap, even such simple sentences as " There is a piece of white paper on the table " need an infinite number of operations and statements to be verified. Empirical laws are not verifiable since they refer to an infinite number of instances while all observation is finite.

According to this view, the difference between universal and particular statements must be minimized. It also follows that the interpretations of the testability criterion in terms of complete verifiability or of complete falsifiability are inadequate and too restrictive. This holds especially for the physical disposition terms referred to above, such as ' magnetic ' or ' electrically charged ';

these are introduced by means of so-called reduction sentences which, in spite of their function as operational links, have the character of partial or conditional definitions. They are not definitions in the strict sense of the word, however; they are fragmentary specifications of meaning. Following Carnap, Hempel (*44*) states explicitly that sentences containing disposition terms cannot be fully translated into statements about observables; they involve " open " terms and require an indefinite series of conditions to be tested.

It seems to follow from these considerations that intervening variables in the sense of MacCorquodale and Meehl, referring as they do to a generalized variety of conditions, are likewise elements of " explanation " even though they are of a less tentative and less " theoretical " variety than hypothetical constructs. Since the examples given by the authors for hypothetical constructs are almost exclusively concerned with physiological explanations of behavior, it is not quite clear whether or not their distinction coincides with that of Hempel (*44*) between theoretical constructs introduced by postulates, on the one hand, and concepts introduced by definitions or reduction chains based on observation, on the other. In practically all of the concrete instances of theory construction in science it is not an easy task to decide whether a given concept has been arrived at primarily on the basis of theoretical considerations or primarily by observation. This holds especially for such a relatively immature theory as psychoanalysis. We would venture to guess that Freud, guided by some relatively fragmentary initial impressions, has arrived at many of his constructs in a process of building a theoretical structure, with empirical interpretation lagging somewhat behind. A careful reading of Freud makes it quite clear that more space is devoted to the definition of theoretical constructs, such as superego, ego and id, in terms of their structural relations to each other than by reference to the relations of these various constructs to observation.

The distinction between intervening variables and hypothetical constructs may to a certain extent be preserved in the face of the above arguments by differentiating between two kinds of surplus meaning. The first kind could briefly be labelled " observational surplus "; it is given by the inexhaustibility of possible tests to be applied in the confirmation of an empirical statement and therefore is present in all intervening variables. The second we will call " existential surplus "; by this we mean primarily the hypothetical transcendence beyond the existential frame of reference in question, such as when a micro-structure is adduced in the explanation of macro-behavior.

The involved character of the above considerations makes it difficult to decide whether in psychoanalytic theory we deal mainly with hypothetical constructs or with intervening variables. For example, the concept of unconsciousness may be classified as a hypothetical construct if it is meant to refer to a theoretical system, not yet determined in physiological terms but with the claim that one day it will be determined. In this case it involves the assumption of an entity which goes beyond the behavioral framework per se and from which many particular occurrences are being derived. On the other hand, if we conceive of the unconscious wish to kill one's father, or the unconscious wish for failure, then we have before us what may be considered intervening variables, since reduction to if-then type of statements seems possible for all cases involving the use of these terms. Many of the psychoanalytic terms are disposition terms; they refer to latent qualities which become manifest only under specified conditions. As disposition terms they are intervening variables in the sense of MacCorquodale and Meehl. As we have seen, however, their definition remains incomplete and open since we cannot specify all conditions and manners in which latent tendencies become manifest. For this reason it may be preferable not to consider a disposition predicate as defined by the total number of reduction sentences formulated at any given stage of a science. The actually stated conditions of testing constitute as a rule but a limited selection at which we may have arrived by an informal process of ingenuity, leaving the possibility open for the discovery of new conditions to be added or to be substituted for the old ones.

Since intervening variables share with hypothetical constructs the fact that excess value is being added to the observed phenomena, their distinction should be viewed as one of degree rather than of principle. However, while Bergmann, in a recent survey of theoretical psychology (*4*) which appeared after the relevant parts of the present paper had been written, goes so far as to obliterate the distinction altogether, the present writer would favor to retain it as a gradual one. Hypothetical constructs are higher-order concepts from which intervening variables can be derived. In Feigl's scheme of explanations, mentioned above, the disposition terms have to do with empirical regularities while hypothetical constructs may furnish the constructions by which the disposition terms are unified. But again we must stress that disposition terms as such already contain unifying principles, although there remains a difference in the degree of accessibility to confirmation which may be ascribed to the heterogeneity of the hypothetical constructs relative to the observations to which they are adduced.

The fact that intervening variables are not fully defined and fully interpreted in terms of observation raises the question as to whether inferential concepts should be employed at all. Against such a restriction we hold that overly specific or narrow concepts are usually not too helpful. Psychoanalytic theory is far from succumbing to this danger. It abounds in both intervening variables and hypothetical constructs the specification of which often falls short of even the more liberal requirements adopted in this paper. However, Freud's use of a mentalistic and phenomenalistic language creates the erroneous impression that he stayed closer to the verbal behavior of his patients than he actually did. These and other phenomena and symptoms were used by him as a basis for the construction of genuinely inferential concepts, although the connecting links between the two levels were not always made explicit. The indicated narrowing of the gap between intervening variables and hypothetical constructs may support our tendency to reduce the stringency of the requirements for the establishment of hypothetical constructs.

Here we must add a few critical remarks concerning MacCorquodale and Meehl's requirement that hypothetical concepts which refer to physiological entities must correspond to our present-day knowledge in physiology. The progress of empirical science would be unduly hampered if all our explanatory terms had to be specified fully in terms of the discipline from which they are taken. Progress in physics has been greatly furthered by the use of often wild-sounding assumptions or models. We have seen that Freud was fully aware of the fact that in view of the state of the biological sciences he could not utilize knowledge from physiology and anatomy; yet he did not hesitate to improve his own models, and was amply rewarded. The hydraulic model of Freud mentioned above led to a fruitful conception of psychological relationships, and such visualizations as that of the dammed-up libido having to use devious channels when blocked led to a deeper penetration of the relationships involved and to improved predictions. Similarly, to think of repression in terms of " keeping something down " so that stronger forces are necessary to counteract the repressed force, leading often to an intensification of the counteractive forces, seems to be a more productive way of thinking about the processes involved than would be a purely descriptive statement about a sequence of certain events. As far as the possibility of an eventual reduction of these analogies to physiological observation is concerned, Freud was, as we have seen, most hopeful. The question of the reduction of psychoanalysis to physiology is, however, an empirical and not a logical one.

In the literature there seem to be differences of opinion as to whether science tends to proceed from hypothetical constructs to intervening variables or in the opposite direction. The question is raised as to which of the two represents the more mature stage of scientific development. It seems that scientists, especially those with great ingenuity, have frequently proceeded from observations directly to hypothetical constructs and have derived the intervening variables later, although sometimes the procedure was reversed. The most fruitful policy seems to be that of an alternation between the cultivation of the images and constructs with all their richness of meaning and a concerted effort toward a more precise observational specification of the terms involved. There can be no doubt that the ultimate function of science is the achievement of these greater specifications and clarifications. For the sake of the continued creativity of the scientific process, however, it is important to keep in mind that the specifications do not always reflect the full content of the original meaning. Thus Freud at first made relatively specific observations about repression of sex instincts; these observations stimulated him to introduce the concept of the unconscious; only after that did he proceed to specify a number of other mechanisms which account for the transformation of unconscious material and which constitute the heart of the Freudian theory. The oscillation between hypothetical constructs and intervening variables seems to afford a protection against both a too narrow operationalism and the dangers of meaningless generalization.

7. Attempts at Confirmation of Psychoanalytic Hypotheses

As we have seen, the elements of overt behavior and of inferred motivation vary to a certain degree independently. But since the underlying motivation is often unconscious and can, therefore, not be verbalized by the subject, and since it also is disguised by the behavioral techniques used by the subject, there are difficulties inherent in the observation and measurement of the underlying motives. By referring to concepts more removed from the immediate data, psychoanalysis has lengthened the chains of intellectual and experimental work which connect the principles with the protocols or observation. We may recall the statement of Philipp Frank (*26*) that in modern physics greater ingenuity is needed to find ways of verifying the theories in question, and that this fact is a result of the greater abstractness of concepts. He has pointed out how easily the statements of traditional Newtonian physics could be verified by observation since they were a direct formulation of our everyday experience, obvious and plausible to common sense. He goes on to explain that

> in Einstein's general theory of relativity, however, the description of the operations by which the quantities involved could be measured becomes a serious and complex task. It becomes an essential part of the theory (p. 19 f).

Psychoanalysis thus shares with physics in the fact that their statements do not lend themselves to direct and obvious confirmation. In either case, the highly interpretive statements involved do not carry the rules of their confirmation as obviously with themselves as do descriptive statements. The situation in psychoanalysis was further complicated by a certain tendency – leading obviously into a blind alley – to attempt an isolation of some of its statements which could only be understood in the context of other statements, and by some futile attempts to find an " experimentum crucis." But there also is increasing realization of the fact that more stress should be placed on the verification of predictions than of the theories themselves.

Several possible ways to circumvent the methodological difficulties involved in the approach to the latent level will now be discussed briefly.

Let us begin with verification attempts based on the therapeutic process. The ego defenses which are at work to prevent the patient from consciously facing many of his basic tendencies or conflicts are being gradually removed in this process so that a more correct picture of the underlying dynamic realities emerges. Many promising attempts are being made to render the therapeutic diagnosis and procedure accessible to intersubjective scrutiny. Recording of the procedure allows submission of the material to more than one observer, a checking of predictions, and a comparison of interpretations and evaluations, the latter being stressed especially by Kubie (*49*).

One of the most important aspects in the objectification of psychoanalytic knowledge in general will be the making explicit of the cues which the therapist uses in his interpretations. At the same time this will help to reduce the subjectivity of the individual interpretative process. Freud's benign yet sophisticated outlook on the dangers of interpretation may be illustrated by the following quotation:

> Our justification for making such inferences and interpolations and the degree of certainty attaching to them of course remain open to criticism in each individual instance; and it is not to be denied that it is often exceedingly difficult to arrive at a decision – a fact which finds expression in the lack of agreement among analysts. . . . For in psychology, unlike physics, we are not always concerned with things which can only arouse a cold scientific interest. Thus we shall not be so very greatly surprised if a woman analyst who has not been sufficiently convinced of the intensity of her own desire for a penis also fails to assign an adequate importance to that factor in her patients. But such sources of error, arising from the personal equation, have, when all is said and done, no great significance. If one looks through old textbooks upon the use of the microscope, one is astonished to find the extraordinary demands which were placed upon the per-

> sonality of those who made observations with that instrument while its technique was young, and of which there is no question today.

Next is the experimental approach to underlying motives. This approach has thus far been satisfactory only in certain specific directions. Since the ego-defenses are nearer to the surface, they have proved particularly attractive for experimental investigation. Thus we find a number of experiments aimed at verifying psychoanalytic statements on fixation, regression, repression, and projection. Sears (*63*) and more recently Hilgard (*45*) have summarized and discussed many of these experiments. It is interesting to note that Sears comes to the conclusion, and we follow him here, that hypotheses on fixation and regression have been better confirmed experimentally than those on repression and projection – and, we would add, better than those on reaction formation. This agrees well with one of our contentions made above. Fixation is indeed a descriptive concept referring either to persistent attachment to a certain love-object or arrest on a certain developmental level. We may refer here to an experiment with newborn rats (*73*). For a period of a few weeks some were blindfolded and the others had their ears stopped. When, after full growth was attained, the rats were put under stress, the first group developed functional seeing difficulties, and the second group functional hearing difficulties. In this experiment, undertaken by A. Wolf, it was possible to isolate early trauma and to show the ensuing fixation and regression. Blindfolding and ear-stopping at a later date did not lead to the same consequences, which is in line with psychoanalytic emphasis on the earliest phases of infancy. The effect of certain infantile frustrations was demonstrated by Hunt (*46*). Rats starved in early infancy were found to eat and to hoard more food in later life than those who had been fed satisfactorily while young. By producing different types of conflict in animals, Masserman (*52*) studied some aspects of the origin of neurosis in animals, and even the effects of what amounted to various types of therapy. Only a relatively small segment of psychoanalytic theory can be effectively tested with animals, however.

The relation of frustration to regression has also been demonstrated in children. Barker, Dembo and Lewin (*2*) first induced a group of children to play with parts of toys, the gaps to be filled out in imagination. Then the children were shown a fuller and more attractive set of toys through a glass window. Their approach to the available toys as well as their general behavior deteriorated markedly after this frustration.

The relative difficulty of studying experimentally such further mechanisms as repression or reaction formation has above been ascribed to the fact that the concepts involved derive from the more inferential and abstract parts of psychoanalytic theory. Repression was originally described by Freud in connection with the handling of threatening id-impulses, primarily those of sex and aggression. Complex conditions, such as those involved in the analysis of transference, are required before that which was repressed may become conscious. Most of the frequently adduced experiments dealing with such facts as memory for overtly pleasant or unpleasant words cannot be brought to bear on psychoanalytic assumptions of this kind, for these are concerned with quite a different matter. Since repression proper extends to the ideational content originally connected with the forbidden instinct, the selection of words which are consciously pleasant or unpleasant for the average person may have very little dynamic relevance for a particular subject. Furthermore, contrary to widespread belief, Freud did not assume that only the pleasant is remembered. In *Beyond the Pleasure Principle* (*37*) he discussed the tendency of children to reproduce anxiety-inducing experiences in play as well as in dreams in order to achieve mastery of the traumatic situation. Misunderstandings of psychoanalytic theory have arisen when in the manner described statements concerning repression which originally were intended to refer to unconscious – that is, inferred rather than overt – processes, are erroneously taken as purely descriptive statements of conscious contents. As in physics, a simple identification of statements containing disposition terms with statements about manifest events is not permissible.

Some of the leading learning theorists, such as Dollard and Miller (*18*) and Mowrer (*53*), have contributed challenging and life-near experiments conceived in the genuine spirit of psychoanalysis. Certain mechanisms, such as regression, fixation, and displacement, were effectively demonstrated by these experiments. Sharing a " historical " outlook with psychoanalysis, the learning approach has highlighted microscopically such mechanisms as reinforcement or generalization which are also implicit in psychoanalysis. However, learning theory cannot completely explain why in certain contexts we learn as little as we do, as psychoanalysis has impressively demonstrated. Norman R. F. Maier's (*51*) assumption of two different mechanisms of behavior, the motivation and the frustration behavior, constitutes a recognition of the same kind of fact; in the system of Maier only motivation behavior is the learning type of behavior, frustration-instigated behavior is not.

The general reason why the typical psychological laboratory experiment is not well suited to the purpose of verifying or disproving psychoanalytic hypotheses is its preoccupation with the isolation of relatively irrelevant variables and with psychological tasks which have little to do with the vital processes psychoanalysis is concerned with and in which even success or failure is usually of little relevance. Much of this preoccupation is avoided in the so-called "projective" techniques with all their subvarieties in which the meaning or story "theme" imputed by the subject to ambiguous pictures is made the basis of interpretation, as well as in certain of the more complex memory experiments and in some further related types of procedure. All these techniques give access to dynamically relevant material and the transformation it undergoes when it passes into consciousness chiefly because they do not insist on the unattainable requirement that the subject drop his defenses to help our effort to study his unconscious. The defenses, as well as the tendencies against which these defenses are erected, are revealed in a more indirect manner. Along the same lines Kubie (*49*) has advocated the study of materials brought forth in free association, in hypnosis, and under the effect of certain drugs.

Still other experimental studies have verified such seemingly far-fetched psychoanalytic assumptions as symbolism. We are thinking here primarily of experiments by Schrötter (*62*) and by Betlheim and Hartmann (7) at the Vienna Psychiatric Clinic in which stories with crude sexual content were told to uneducated hypnotized subjects and to Korsakoff patients. These stories were dreamt about or reproduced in memory with the spontaneous use of symbols for sexual content in conformance with standard psychoanalytic assumptions. A similar study was conducted by Brenman (*10*), who asked her subjects to recall fairy tales which they had heard during childhood. The reproductions showed omissions and distortions in the direction of the emotional needs of the subjects as ascertained by the Thematic Apperception Test (*54*) and by hypnosis.

Turning now to the genetic approach, the study of psychosexual development in its various stages assumed by psychoanalysis (oral, anal, phallic, etc.) has been attempted by Blum (*9*) with the use of twelve cartoons depicting a dog, named " Blacky," as engaging alone or with his parents or siblings in all the activities assumed to be relevant for the various libidinal stages. The interpretation of the cartoons has been found to correlate with the psychodynamic pattern of the subject. Again, the subject is less on guard and less apt to protect himself, describing as he does the activities of a dog rather than those of a human. Still, the interpretation of the Blacky-test remains much more uncertain than the author assumes.

It has been pointed out by many observers that although psychoanalysis is mainly genetically oriented and explains much of adult behavior in terms of early childhood experiences, most of this knowledge is based on a reconstruction from the therapy of adults rather than on direct observation of the child. Such a classic psychoanalytic summary of the development of mental and instinctual processes as that by Fenichel uses almost exclusively examples from neurotics and psychotics to discuss what happens in childhood. Observation of children would seem the more fruitful, as the instinctual processes have not yet undergone the transformations which are supposed to occur under the pressure of family and social institutions. The ultimate requirement for a dynamic child psychology remains the ascertainment of the interrelationships of early childhood events to later personality structure, and only a so-called " longitudinal " study planned under the auspices of genuinely psychoanalytic modes of thought and extending over several decades could give an adequate answer. How to get hold of all the events which acquire meaning for the growing child and how to interpret these events, especially in the pre-verbal stage, still offer staggering methodological difficulties.

8. Formal Criteria in a Study of Self-Deception

In an experimental study of mechanisms of self-deception this writer (*27*) has attempted to make the ego-defenses themselves the focus of exploration. Self-descriptions – as well as asserted " guiding principles " for conduct – were compared with descriptions of the subjects' actual behavior by detached observers well acquainted with them. The distortions in an individual's self-appraisal were found to be greatest in the areas of actual behavioral shortcomings. The defense mechanisms of the type described in this study can be shown to be in a formal sense very similar to those described in psychoanalysis, although the emphasis is mainly on the defenses against loss of self-esteem rather than on defenses established in early childhood against instinctual dangers. Projection, for example, is in both cases defined as the ascribing to the environment what is internal and at the same time rejected. In the area of overlap with psychoanalysis lie especially our self-deception mechanisms of distortion into the opposite, minimization, rationalization, and projection. These mechanisms make it possible for the individual to see himself, say, as highly sincere even though he is judged by others to be highly insincere.

The findings of this study help to throw light on the problem of the function of consciousness in behavior. The inverse signalizing given by the mechanism of distortion into the opposite is frequently also expressed in the so-called guiding principles of conduct, for a report of which we had also asked. The ultimate function of this inverse signalizing in consciousness, especially in its latter form, is probably the modification of behavior. We find, furthermore, projective types of distortion, exemplified by a subject judged to be very aggressive who at the same time demanded that others be more friendly. One of the subjects rated as lacking in self-discipline demanded a much stricter organization of the institute to which he belonged. Thus sometimes the function of consciousness seems to be of a defensive and compensatory character, protecting the individual from a painful truth which he has been unable to face. This is in harmony with the general view psychoanalysis takes of consciousness. In other cases we found a conscious realization of shortcomings in oneself which were perceived by others not until later because they were hidden behind a well-functioning façade.

Our next step was to establish criteria by which we could ascertain whether in a given case the underlying motivational and behavioral realities were distorted or faithfully mirrored in consciousness. This brings us to certain semantic aspects of our results and their diagnostic implications. For example, we found that self-descriptions which do not correspond to the social realities are formulated in an exaggerated way. This exaggeration was found to be symptomatic of the absence rather than the presence of the asserted desirable trait. The use of such linguistic devices as superlatives, generalizations, and repetitions was found to be statistically concomitant with a shortcoming rather than a strength in the area concerned. The greater the definiteness and lack of shading and the greater the intolerance of ambiguity in the self-description of favorable traits, the less often as a rule were the traits concerned found to be present according to the judgment of close acquaintances. A list of " formal " cues of this kind, derived from our material (including some later studies) and useful in the interpretation of the truthfulness or sincerity of behavior or of self-reports, include the following: exaggeration; absoluteness of emphasis; intolerance of ambiguity; predominant reference to the extreme values of what actually is a continuum; inconsistency of general with specific features in behavior or in verbal description, or of responses to direct as compared with indirect questions; the occasional breaking through of a pattern of denial; deviations from the perceptions, concepts, and other answers which constitute the norm for the group as a whole; stereotypy; over-concreteness; small range of variability in response; repetition; and all manner of rigidity.

It may be assumed that these formal characteristics are less subjected to censorship than is the type of instinctual content studied in psychoanalysis proper. They may, therefore, in certain contexts be of greater diagnostic value than are symbols and other direct disguises for content elements.

In the approach just described we have taken a path around the individual's self-perceptions and around the disguises his basic tendencies undergo in his own awareness or overt behavior by having other observers evaluate the dynamic realities of the subject on the basis of his overt behavior. Another method relies entirely upon the statements a subject gives about himself but involves such a broad matrix of his statements that we are enabled to discern, with the help of the formal semantic criteria derived from the previous type of approach, whether or not the behavioral realities are consciously or unconsciously distorted. An example of this procedure is the interpretative evaluation of interview material. Thus the authoritarian child tends to begin with assurances of great admiration and love for the parents in reply to rather general questions but proceeds to reporting episodes of victimization and injustice in answer to more specific questions (*30*). A somewhat similar inconsistency is given by the case of an ethnically prejudiced child who, though relating only positive feelings while talking directly about his parents, omits his parents from the list of persons he would take with him to a desert island.

In comparing the interview with the projective techniques concerning their power of penetration to the dispositional layer, we find that while in some individuals, especially in those in whom underlying tendencies are near the surface yet not quite conscious, the projective material seems to highlight certain dynamic tendencies in a particularly direct and dramatic fashion, other individuals appear definitely less differentiated on projective tests than they do in their reactions to direct interviews.

9. Psychodynamics and Cognition

One of the areas best suited for the testing of some of the psychoanalytic hypotheses as well as for the expansion of our knowledge of dynamic ego-psychology is that of perception and of cognition in general.

In academic psychology, perception and cognition were first approached under the aspect of the universal laws of general psychology including psychophysics, and with an emphasis on a cognitive " task " and the subject's way of solving it. In the foreground of attention stood the adaptation to reality. Individual differences were at first neglected, and their tracing back to the varying histories and motivations of individuals has hardly come into its own even today. For a long time the dominant tradition was that of association psychology with its conception of the human mind as a *tabula rasa*. *Gestalt* psychology was the first to introduce an organismic factor of cognitive organization, but this in turn was derived from the assumed general structure of the physiological apparatus rather than from more specific genetic sources, be they intellectual or emotional. Thus the cognitive processes continued to appear mainly as transmitters of characteristics of the physical stimulus, and all distortions appeared as reflections of certain characteristics of the cognitive system itself.

Beginning in the nineteen hundred twenties there appeared on the German psychological scene typologists such as Kretschmer (*48*) and Jaensch (*47*) who claimed the existence of over-all styles of personality which would be manifested in a variety of cognitive patterns. Investigators in this group likewise tended to pass by motivational factors. Related to this typological approach is the approach via the pathology of thinking as represented by Bleuler (*8*), Kurt Goldstein (*42*), and Norman Cameron (*14*). Under these auspices pathological, especially schizophrenic, thought patterns are described primarily in their illogical properties or as lacking the appropriate abstractness. It is pointed out how symbols are treated as realities, and how thought processes show instability of organization " with a relative inability to exclude contradictory, competing or irrelevant reactions (overinclusion)" (Cameron). In still another related approach the emphasis is on developmental aspects of cognition. Stages of thought patterns are differentiated mainly from the standpoint of coherence of organization or adequacy to reality, and seen as determined by maturation. Piaget (*56*), Heinz Werner (72) and others stress the egocentric, syncretic, diffuse, physiognomic, rigid and at the same time labile character of the thought processes in the child or the immature person. These latter descriptions begin to resemble the notions concerning primary processes as entertained in psychoanalysis.

Still further, and more distinctly under the influence of psychoanalysis, are the studies dealing with the influence of motivation on perception, often summarized under the label of autism. Yet it must be remembered that within academic psychology autism in motivation and cognition is on the whole not conceived as the result of early psychosexual experiences and dynamic mechanisms; the emphasis is mostly on temporary or otherwise personality-alien factors defining a single motivational force in relative isolation. Such forms of motivation as want, fear, experimentally-induced reward or punishment, and other social values are superimposed upon a problem rooted in the tradition of the general psychology of cognition. In all the approaches listed so far in this section we find what may be called a " cognition-centered " orientation, that is, a basic dependence on the outlook, problems and techniques developed in general psychology or descriptive psychiatry.

In the context of psychodynamics proper, on the other hand, we must think of a quite opposite type of approach, one in which problems are primarily organized about our notions of the motivational core of the person and are only secondarily extended to the field of cognition. This kind of approach we may call " personality-centered " (*30*). Such an orientation lays stress on the irrational, unconscious and archaic layer of the personality, the discovery of which constitutes the major contribution of psychoanalysis in its basic function as depth psychology.

As mentioned above, Freud was well aware of the vicissitudes of the id under the influence of reality. Still, he viewed the functioning of the ego and character structure under the aspect of defensiveness more than under that of adaptiveness. Although it is assumed that in the course of development an objective, reality-adequate approach supersedes the primal undifferentiated, wish- and fear-ridden outlook, the primary process is not assumed to lose its dynamic significance; according to Freud " the voice of the intellect remains low " even though it is also seen as " persistent." Taking off from Freud, several psychoanalysts have formulated challenging hypotheses concerning the relationship between thought patterns and underlying dynamics, however. These have far-reaching implications on the timely problem of the psychodynamics of acceptance or rejection of scientific theories.

We may illustrate this by means of Theodor Reik's (*59*) cogent analysis of ecclesiastical dogma. According to him, the dogma of the trinity is a compromise formation which achieves the unification and reconciliation of what appears to be a logical contradiction. This dogma makes it possible to award Christ, the son, a position beside or really above God, the father, and at the same time to preserve the unique position of God as the father. Reik believes that in every dogma there is an attempt to unify opposing impulses; in the historical development of dogma there is an alternation between the prominence of the repressing agencies and the return of the repressed ideas. The emphasis placed on the certainty of the dogma originates in the defensive battle against doubt and heresy. As in the obsessional idea, the mechanisms of dogma are those of generalization, displacement, isolation; the transfer of doubt to insignificant detail serves to prevent the latent meaning of the obsessional idea or of the actual dogmatic statement from becoming conscious. Contradictions in both dogma and obsessional thinking can be explained by the oscillation between two opposing poles or by ambivalence which, as Freud has pointed out, plays so large a part in obsessional neuroses. There is a stress on the differences resulting from the privacy of the obsessional ideas, which are usually kept hidden or unconscious, and the proclamation of religious doctrines, which endeavor to become official and public.

In his detailed phenomenological account of the thought patterns and total approach to life of the masochistic personality (*58*), including the cultural aspects of masochism, Reik stresses the significance of fantasy, the suspense factor and the demonstrative and provocative features of this syndrome. In consequence of his lively imagination the masochist is prone to anticipate punishment and the anxiety connected with it and thus tends to prolong the suspense. Masochism is seen as induced by social forces and by their intimidation of the ego in fantasy. The masochist seems to acknowledge the demands of reality but at the same time he defies them by an exaggerated obedience which turns the meaning of the demand into its opposite.

Fenichel's (*24*) hypotheses concerning certain patterns of perception in compulsives (some of which were empirically verified by B. Rosenberg in a recent University of California doctoral dissertation) have considerable bearing upon the present discussion. Over and above such relatively well-known features as magic thinking, the mechanism of isolation, displacement onto small detail, and the compulsion to doubt, Fenichel considers the " inhibition in the experiences of *Gestalten* " an important characteristic of compulsion neurotics. He points out in detail how isolation separates constituents of a whole and prevents the compulsive person from awareness of the whole, and how the thinking in compulsive categories represents a caricature of logical thinking since isolation does not serve the purpose of objectivity but rather that of defense. In order to exclude the possibility of surprise, compulsion neurotics have a tendency

> to make false generalizations, to classify hastily all ideas into certain mutually exclusive categories and then to get into a state of doubt concerning the nature and evaluation of the categories.

In emphasizing the predilection of compulsives for symmetry, Fenichel draws on the work of Ferenczi and Schilder. All these characteristics of perceptual and of thought processes are seen in the context of such instinctual orientations as ambivalence toward authority and the wish to keep a balance between the id and the superego.

In any application of the multi-layer approach to thinking we are bound to go beyond the face value of the phenomena and to be sensitized to such formal principles as that of the closeness of opposites, hinted at earlier in this paper. This closeness is the direct result of the modification of the original aim by defense mechanisms. Freud himself (*34*) has dealt with the reversal of instincts into their opposite. He speaks of such changes as the replacement of a passive aim – for example, to be looked at – by an active aim – that is, to look at. The reversal of a content, as found in the change of love into hate, constitutes a similar pattern. Our mental life is seen as governed by three polarities, subject-object, pleasure-pain, activity-passivity, in each of which one pole may be replaced by its opposite.

In my own work on cognition, closeness of opposites could repeatedly be observed. A certain inability to tolerate complex, conflicting, or open structures was observed in the perceptual and cognitive approach of ethnocentric and authoritarian individuals (*30*). This "intolerance of ambiguity" might, it seemed, be repeated in the more purely emotional sphere in the same manner as it is found in the social area. Ranking our subjects according to their intolerance of emotional ambivalence and their tendency toward perceiving others in terms of positive or negative halos and dichotomies rather than allowing for independent and continuous variability of traits, we attempted to ascertain just how pervasive this intolerance might be. This was done in a number of experiments on memory, concept formation, and perception proper.

Results so far collected support the conjecture that such tendencies as the quest for unqualified certainty, the rigid adherence to the given, the inadequacy of reactions in terms of reality, and so forth, to a certain extent tend to spread between the various areas of personality. It can be demonstrated, further, that such specific forms of reaction as "stimulus-boundness," that is, the pedantic orientation toward concrete detail, tend to reoccur within an individual in contexts seemingly far removed from each other. Inclination toward mechanical repetition of faulty hypotheses, inaccessibility to new experience, satisfaction with subjective and at the same time unimaginative, over-concrete or over-generalized solutions, all appear to be specific manifestations of a general disposition. It seems to matter little whether the authority to which there is submission is that of a person or that of a physical stimulus. In each case the choice seems to be between total acceptance and total rejection; if the two overlap, they do so in different layers of the personality. Avoidance of conscious coexistence of acceptance and rejection which could lead to qualified feelings and statements, and of other complexities is confined to the surface level, however, with chaos lurking behind and breaking through the rigidly maintained façade. With internal conflict being as disturbing as it is in the rigid person, there apparently develops a tendency toward denying external ambiguity as long as such denial can be maintained. The clinging to definite dichotomies and demarcation lines apparently reduces the conflict at the conscious level but at the same time increases the underlying confusion. In the non-authoritarian type of intellect, on the other hand, the total pattern is that of a broader integration of reality; no parts are left out, and thus a more flexible adaptation to varying circumstances is achieved.

It is the lack of integration and the resultant break between the conscious and unconscious layers in the rigid, authoritarian person, as compared with the greater fluidity of transition and of intercommunication between the different personality strata in the more flexible individuals, which appear to have the most far-reaching implications for the respective personality patterns. The shutting out of certain aspects of feelings, and of the inner strata in general, must be seen as the source of the distortion of the external perceptions and judgments. In spite of the recurrence of common elements in various areas, there is no obvious or simple "unity of style " in this type of personality, at least not so long as we take this term in the sense in which it is used by such typologists as Jaensch (*47*). At least in part this is the result of the many repressions and of the break between the conscious and unconscious levels discovered and explored by depth-psychology (*34*). Thus there is a coexistence of such seemingly incompatible opposites as rigid perseverative behavior with an over-fluid, haphazard, disintegrated, random approach, of compulsive over-caution with the tendency toward impulsive shortcuts to action, of chaos and confusion with orderly over-simplification in terms of black-white solutions and stereotypy, of isolation with fusion, of lack of differentiation with the mixing of elements which do not belong together, of extreme concreteness with extreme generality, of cynicism with gullibility, of over-realism with irrationality, of self-glorification with self-contempt, of submission to powerful authorities with resentment against them, and of stress on masculinity with a tendency toward feminine passivity. But it must be remembered that, say, stimulus-boundness, that is, a pedantic orientation toward concrete detail, does as little justice to the problems and principles inherent in any cognitive task as does its opposite, over-generalization. An abstract attitude stressing overall principles is better suited for the penetration of the structure of the concrete and particular data than is exclusive attention to these data in their concreteness. A concentration on the literal meaning of a stimulus datum often entails a distortion of its essential contextual characteristics. The same situation obtains in science where the imaginative, subjective, or intuitive elements with which facts are viewed are of crucial importance for their intellectual mastery.

Styles of thought both in philosophical and scientific thinking must be assumed to be determined by both psychological and social factors. For instance, in psychology an altogether different type of person is usually attracted to the psychoanalytic system from those who show signs of wishing to escape into the palpable, concrete and obvious forms of thinking. Besides the emotional resistance to psychoanalysis, the entire problem of the remoteness from data must be considered here. While some scientists feel safest when they are very close to the data, others feel safest when there is no reference at all to factual data; still others can tolerate the medium levels of abstraction. Prejudgments of an emotional and cognitive nature enter the picture. The problem of social factors is especially acute now, when we are witnessing a new upsurge of the search for absolutes in the wake of the anxieties and tensions inherent in our time. There can be little doubt that the choice between such philosophical positions as idealism, rationalism, and empiricism is likewise dependent on underlying personality factors. The same may be said for such ethical positions as absolutism, relativism, pessimism, optimism, and so forth.

A further approach to the depth-surface relations which this writer has followed up in some detail is the evaluation of the stories given in response to Murray's Thematic Apperception Test referred to above (*54*) in the direction of relating the dynamic conflicts to formal characteristics of imagination and thinking. There is evidence that good intellectual organization of the stories often goes with a medium degree of aggression. Underdeveloped aggression tends to impair intellectual penetration and overdeveloped aggression tends to lead to disorganization. The evidence so far collected seems further to suggest that advanced psychosexual development in pre-adolescence, e.g., the prominence of the Oedipal conflict, tends to go with realism and originality, and that the coloring of the personality structure in terms of orality or anality is a further determinant of the formal quality of the production. Thus one of the adolescents under study who had progressed as far as the Oedipal stage but not beyond it and who at the same time still manifested some of the genetically earlier oral traits displayed marked imagination and creativity in the sense of a true grasp of reality, whereas the more purely infantile orality in another boy was accompanied by a sterile type of rumination about the topic of food. Pre-adolescents who are relatively mature in their psychosexual development tend to produce either imaginative fairy-tale-like stories which show creative understanding of real relationships at a symbolic level or very realistic stories which reveal differentiated cognition. Children who display intolerance of ambiguity and are fixated at earlier stages of development tend to display either impoverishment of thought processes or unrealistic stories full of rambling repetition which do not add up to any organized sequence of events. All this is not to say that certain abilities may not develop to a certain degree independently of instinctual and emotional conflicts. It is not only the drive which must be assumed to determine the fate of ability but also the ability which must be assumed to determine the fate of drive (*28*).

In conclusion, the vastness of the area of cognition and of imagination in which the contribution of instinctual vicissitudes and of internal mechanisms in general is crucial must not be underestimated, however. The internal dynamisms which psychoanalysis has described mainly in the domain of sex and aggression must be assumed to reach into all fields of psychology, including the seemingly remote one of scientific cognition. Over and above the external observations and the given abilities to organize and to create, our thinking is undoubtedly strongly determined by such defense mechanisms as isolation, repression, displacement, rationalization, and so forth. From our own work we reported interrelations between an ego-alien and not accepted ambivalence toward parental figures, on the one hand, and an intolerance of cognitive ambiguity coupled with rigidity, on the other. Psychosexual development in general seems to be an important determinant of content and quality of imagination. A complete picture of personality must refer to both the pattern of basic motivations and to the manner of manifestation of the motivational tendencies. For prediction we need both depth and surface. While psychoanalysis has been asking, " Which drive? ", and general psychology has been asking, " Which effect? ", a unified psychology must ask, " Which effect out of which drive? " (*28*). To a certain extent Allport's (*1*) principle of " functional autonomy " may hold, that is, there may be partial independence of cognition and social adjustment from genetic and instinctual factors. Emphasis must at the same time be laid upon the interrelationships between the two areas, however, and upon a multi-layer approach to all psychological phenomena. The psychologist may be able to contribute to our insights into these depth-surface relationships considerably beyond what psychoanalysis has developed so far. With an added knowledge of underlying dynamics we will be the better equipped to identify and to integrate over-all organizing principles of overt adjustment. Thus the phenotype is reintroduced without losing sight of dynamic aspects. We must take the detour over the genotype to understand the phenotype. Freud (*40*) was clearly aware of this fact, and this was the reason why he did not regard psychoanalysis a closed system. Rather, he conceived of psychoanalysis as needing other types of psychology to complement it:

> By itself this science is seldom able to deal with a problem completely, but it seems destined to give valuable contributory help in a large number of regions of knowledge. The sphere of application of psychoanalysis extends as far as that of psychology, to which it forms a complement of the greatest moment.

10. Psychoanalysis, Ethics, and Rationality

Since the true or alleged ethical implications of a scientific theory tend to determine its acceptance and further destiny, any discussion of the scientific involvements of psychoanalysis must remain incomplete without a consideration of its ethical relevance. It has frequently been objected against psychoanalysis – although perhaps more often in the past than in the present – that its orientation is fundamentally unethical and that it aims at the dissolution of morals. Arguments of this kind were raised not only by philosophers in search for a system of absolute, transcendental values but also by empirically oriented social scientists and psychologists of major stature, for example Max Weber (*70*). According to his wife and biographer, Marianne Weber (*71*), Weber saw in psychoanalysis an expression of a tendency to loosen up our basic ethical principles. She gives a vivid picture of how they both were impressed by the necessity of the acceptance of ethical norms, laws and obligations, and how disturbed they were by any movement that seemed to challenge this necessity. In this context we find a number of references to Freud. In a long letter written in 1907 Weber had tried to justify his negative attitude toward an article of one of Freud's followers in which reference is made to Freud's system as a psychiatric or " nerves " ethics characterized by the prevalence of the hygienic point of view (*71*, p. 417). It is essentially the claim, made by some of the followers of Freud, that psychoanalysis represents a new kind of ethics which Weber finds objectionable.

Weber also criticizes the application of psychoanalysis to religion (the major works of Freud on this topic had not yet appeared at that time). Indeed, Freud's predilection for genetic and biological explanations of social and cultural phenomena including ethics can best be illustrated by his conception of religion. The reader will remember our reference, in the preceding chapter, to the views of Reik on religion which in essence are similar to those of Freud. Freud points out that the search for consolation in the face of threatening feelings of inadequacy and helplessness to which religion provides the answer is nothing new in the life of the individual, since everyone had found himself in a similar situation of helplessness as a child vis-à-vis the parents. The longing for a strong father is seen as closely related to the longing for God. The wisdom and goodness which are attributed to the deity reduce anxiety concerning the dangers of life, and moral order and justice are secured in phantasy even though they may rarely be fulfilled within existing human cultures.

According to Freud the substitution of the scientific spirit for the earlier religious beliefs is a rather complex and long-drawn-out process. Freud favors a purely rational justification of cultural commands as a social necessity. In anticipating objections to this view Freud points to the apparent contradiction between his assumption of the domination of the individual by instincts and passions, on the one hand, and his substitution of reason for emotion as a basis of obedience to culture, on the other. He tries to dispel these anticipated objections by explaining that the intellect is our only means of mastering our instincts, and that in turn mere threats and anxiety are rather ineffectual in the strengthening of intellectual functions. Again we are reminded that the voice of the intellect, though low, does not cease until it is heard.

While Freud tends to explain such social phenomena as religion primarily by reference to dynamics within the individual, most anthropologists and sociologists, notably Durkheim (*19*) and Max Weber (*70*), prefer explanations in terms of processes conceived as involving society as a whole. In Durkheim's writings there is an emphasis on respect toward religion as an essential concomitant of respect toward normative rules, customs, and basic values. Religious rituals are seen as an expression of common values and of the moral unity of a society. We may refer to Catlin's remark (in his Introduction to the English translation of Durkheim, *19*) that " unlike Freud he (Durkheim) does not make this God a projection of the individual consciousness or nature."

A similar contrast exists between Freud's view that the moral level of a group can only be preserved when the group keeps the characteristics of the individual, and that of Durkheim who assumes that there is nothing moral in the individual as such; it is society in its own right which is possessed of superior moral authority.

To both Durkheim and Weber religion, far from being seen as an illusion or epiphenomenon, appears as a formative factor in social organization. Weber's analysis of the relationships between capitalism and Protestant ethics (*70*) is but one of the several examples offered by him to illustrate the power of religious ideas. Weber assumes that religious feelings, interests and experiences are worked out differently in different societies; and he is mainly interested in the rational consequences of these religious ideas for social and economic action. Although he gave much attention to an analysis of the rational aspects of organizations like bureaucracy, ultimately this rationality is traced to religious ideas outside of reason.

Both Durkheim and Weber have repeatedly been accused of rationalism albeit the fact that both see the foundations of society in fundamentally non-rational moral qualities. Freud, on the other hand, has been criticized for having given too much prominence to the irrational, while in fact his one hope is the overcoming of the irrational in a society built on reason. Here is an illuminating reversal in the role played by reason when we proceed from the direct verbal formulations made by the authors mentioned to an analysis of the actual function of reason in their theoretical edifices. Freud neglected to explore reason directly and challenged the potency of reason in guiding human conduct. But in his evaluations of the goals of human development he has an exalted esteem for reason, and his understanding for the vicissitudes of unreason has sharpened his grasp for the fundamental nature of reason; in this more crucial respect he is a believer in reason in the best sense of the word. Weber and Durkheim, on the other hand, appear primarily – although by no means exclusively – oriented toward the rational aspects of social conduct. However, one cannot but feel that their grasp of the power potential of reason in human affairs is blunted by an over-rarified idealization of the interplay of rational forces. There is a resulting uneasiness about reason, perhaps not atypical of the twilight of the enlightenment philosophy and of the Victorian age; in the end we find group-cohesion and morality seen as hinging upon emotive patterns.

The ultimate recognition of reason by Freud notwithstanding, the original concentration of psychoanalysis on the discovery of the dynamic importance of the primitive, unorganized system of instinctual drives and of their derivatives has led – as was mentioned above – to a far-reaching neglect of consideration for social and interpersonal phenomena. It is largely this diversion of attention from the functions of reason in psychoanalysis which has given the semblance of ethical relativism. Actually, as we have seen, psychoanalysis was so overwhelmed by its epoch-making discovery of the role of irrational forces that the explicit exploration of reasoning processes appeared as the lesser challenge by comparison even though it was reason and not the irrational that held the top spot so far as the evaluative attitude of psychoanalysis is concerned.

Even within all the reservations which psychoanalysis has voiced against an overly naïve rational interpretation of ethics, it merely turns against the assumed major executive principle of the traditional forms of ethics rather than against the basic constructive content of ethics. This particular executive principle is the mechanism of repression. Most pre-psychoanalytic ethical systems assume that socialized behavior is largely dependent upon such inhibitory devices as the looking away from evil, or its denial, or the mastery of its most blatant overt manifestations through strength of will. From psychoanalysis we have learned about the inefficiency and the dangers of these various forms of repression; and we have also understood why emphasis on sainthood and the techniques of the inquisition so often go hand in hand.

As far as both the goals and the effective means of execution of ethics are concerned, psychoanalysis lays stress on the importance of consciousness, integration, and maturity. If we recall for a moment all that is considered an essential ingredient of maturity in psychoanalysis, such as rationality, the overcoming of aggression, the development of cooperativeness, the ability to love and to work, and the courage openly to face inside and outside threats which oppose these characteristics, we readily see that we are confronted with standards which are certainly not lower than those expounded in the traditional systems of ethics. In psychoanalysis, every neurosis is *ipso facto* considered as failure at satisfactory moral control. The traditional systems of ethics attempted to strengthen consciousness and conscience against the invasion of instincts, and that remains their important historic contribution; however, through psychoanalysis we have become aware of the fact that such strengthening can only be achieved by facing and by working through, rather than by merely condemning, the forces which threaten our conscious personal and social values. From this latter viewpoint, the mortal sin is self-deception, and lack of insight in general, rather than a lack of repression.

It is one of the greatest and least appreciated contributions of psychoanalysis to have seen that for genuinely ethical behavior not only the instincts, so far as antisocial, must be made conscious and integrated into a more encompassing system, but that the major controlling instance of the primordial id-impulses and thus the alleged major guardian of morals, the superego, also may be a source of unconscious sadistic and primitive tendencies. Thus not only the id but also the superego must in the end be subordinated to the more reasonable prescriptions of the ego.

The noxious aspects of the superego derive from the internalized and archaically interpreted or distorted cultural taboos. To use an expression of Fenichel (*24*), in the superego the ego is confronted with an often " irrational representative of reality." Thus, if the neurotic individual keeps punishing himself for sins he has never actually committed, say, for mere feelings of aggression, a total relativism concerning real social action may easily develop. Unconscious moral tendencies stemming from rigid repressions generally turn out to be unrealistic, unadaptable, infantile, and out of touch with social realities. They represent a mere caricature of moral standards which may become destructive of the individual and of his society. These destructive effects of repression may occur in several different ways, frequently combined with each other. The desired yet unaccepted instinctual impulses may remain split up and unmodified, pushing to return and to break through the brittle defenses. Or an irrational superego may remain ego-alien and non-integrated with the conscious personality, thus perpetuating a pattern of conflict and tensions which may easily undermine the conscious and social intentions of the individual as well as his self-esteem. There may, finally, be a tendency to project the guilt stemming from a severe superego onto others; this may lead to the establishment and the persecution of scapegoats in social behavior, or to inequities in the objective appraisal of novel types of approach or theory in scientific behavior.

It hardly needs to be made explicit that rationality cannot be construed to imply amorality and freedom from obligation. On the contrary, genuine ethical behavior involves a comprehension of the issues involved, a facing of all uncertainties and conflicts and of one's own guilt, and a readiness to accept the anguish involved in such an open confrontation. Irrationality and the tendency toward destruction of one's self and of others, on the other hand, are often combined with a short-range over-realism and an orientation toward immediate material benefits. Similarly, steering clear of a compulsive quest for certainty in science does not imply cynicism and morbid doubt. On the contrary, obsession with certainty is a characteristic of irrational problem solutions, and the need for absolutes turns out to be combined with basic disbelief and general distrust.

Some recent writers seem resigned to the fact that only what is irrational, absolute and dogmatic is capable of inciting enthusiasm and of motivating to action, whereas the rational, many-sided approach is seen as inherently inhibitory and as leading to a barren and sterile conception of life or of science. Against this we must hold that individuals who are more open to reason and to facts are in general at the same time those who have a more differentiated internal life and deeper and more reliable – though often relatively calm – emotions. They are at the same time those who, even though less fanatic and less compulsive, show more consistency, conviction and dedication in their principles and ideals. In application to scientific discourse we may stress that extreme or highly obvious positions lend themselves more readily to compact verbal formulation and thus give the false impression of eliminating the perplexities of an unresolved problem situation. Such definite and unqualified statements are especially suited for being put into the service of either very concrete or else excessively general assertions. In the task of the positive formulation of a theory, on the other hand, we must face the difficulties intrinsic in the complexities, ambiguities, flexibilities and less fetching logicalities of the structures at hand, in a manner well exemplified by the stark yet subtle, daring yet not wayward, policies of psychoanalytic theory construction.

As far as conduct proper is concerned, the distinction between the manifest and the latent, which is one of the most basic achievements of psychoanalysis, provides us with the tools for a differentiation between genuine and spurious ethical behavior, even though the latter may be hidden behind a moralistic façade. But psychoanalysis does much more than that; it also provides us with an understanding of the sources of these forms of behavior, by pointing to their roots in relatively early phases of childhood, notably in the degree and type of identification with the parents and in the conscious or unconscious, hostile or loving, character of the relationship established with them. When Freud says that " what began in relation to the father ends in relation to the community," he may have put his finger on one of the most crucial determinants of our social and moral attitudes. The contribution psychonalysis has made to a deepened understanding of the ethical behavior of individuals can be expanded into important insights into the historical development of the great systems of ethics. The need for a metaphysical system of absolute values, the insistence on the freedom of will, as well as unbridled relativism and cynicism, are set in their proper context within a system of personal need-dynamics under psychoanalytic scrutiny.

SUMMARY

An appraisal of the scientific legitimacy and operational status of psychoanalytic concepts (Chapters 1, 2, 3, 4) must first consider certain fundamental changes in the views concerning theoretical structure which have taken place within physics itself as the model discipline for the unity of science and for operationism. Both Philipp Frank and Einstein have pointed to the ever-widening gap between observation and theory; there is increasing realization of the fact that the basic concepts and principles of science must be formulated in an abstract, " non-pictorial " language which seems to belie its origin in the world of direct perceptual experience. Much of the seeming absurdity of psychoanalytic assumptions is resolved by setting them side by side with established physical constructs which in many cases are as much in opposition to the perceptually given as are those of psychoanalysis to the data of manifest, " phenotypical " introspection.

An attempt is made to demonstrate that such basic psychoanalytic concepts as that of the unconscious or of the instinct are either theoretical constructs introduced by postulates, or are concepts defined by reduction-chains. According to Carnap, the latter designate " dispositional predicates." Originally the concepts of the conscious and unconscious signify particular systems possessed of certain dynamic characteristics, calling for a specification of their relationships within the overall formal model. Dreams or subsequent free associations are used for the establishment of intermediate links which can be inserted in the gap between the two systems and with the help of which we can recover the latent material in a process of interpretation.

Freud can be shown to have been familiar with the basic outlook of the philosophy of science. This is evident in his acknowledgment of the fact that the major function of his constructs is the filling out and the integration, in the manner just indicated, of the " exceedingly defective " data of consciousness, as well as in the judiciousness of his use of partially defined or temporarily unspecified hypothetical model-systems. In doing so, Freud stresses that only by means of the assumption of the unconscious can the laws of the conscious processes be established, and that it is this very assumption which enables psychoanalysis to take its place as a natural science. His creative scientific daring is maintained in the face of, and in essential keeping with, the basic requirements of scientific empiricism rather than, as some of his critics would have it, in ignorance of these requirements or in wanton arbitrariness. The far-reaching influence of Helmholtz's physicalism and of the physiology of the late nineteenth century upon Freud has been demonstrated by recent students of the history of psychoanalysis.

While the psychoanalytic system comes closer to a truly scientific theory than most observers realize, psychoanalysis still contains many metaphors, analogies, and confusions between construct and fact which must in the end be eliminated.

The psychoanalytic concept of instinct is complicated by the assumption of far-reaching transformations and disguises, particularly of the sex instinct. But again Freud pursues an essentially operational course when he points to the capacity of the instincts to " act vicariously for one another " and readily to change their objects. It can be argued that the explanatory value of the instincts lies in this emphasis on the variability of their activity, of which the mechanisms of repression, of reversal into the opposite, or of sublimation are some of the more striking examples. Only in the case of an assumed strict correspondence between instinct and manifest behavior would the concept of instinct become circular or superfluous as an unnecessary duplication of behavior.

When Freud ascribes some of the difficulties in his speculations concerning the instincts to our being obliged to operate with " metaphorical expressions peculiar to psychology " we must add in his behalf that for the type of problems with which psychoanalysis deals the mentalistic (introspectionistic or animistic) vocabulary constitutes the precise counterpart to the pictorial vocabulary which has been stressed as a legitimate or at least tolerable ingredient of the earlier stages of physical science.

One of the most bewildering aspects of psychoanalytic theory is the turning away from the obvious face-value picture of personality as it derives from introspection or from the direct, " phenotypical " observation of external behavior segments. An example is the reinterpretation of overt friendliness as a sign of underlying hostility, or of extreme tidiness as a sign of preoccupation with dirt. The discrepancy disappears with the specification of a set of fixed or variable conditions which determine when overt behavior is to be interpreted as genuine and when as manifesting some heterogeneous latent factor.

Since scientific inference concerning central processes, that is, the assumption of internal states on the basis of external evidence, cannot be defended unless it is based on a wide variety of circumstantial evidence, central inference can be said never to have been legitimately attempted before psychoanalysis. It can be shown that on the negative side of the ledger psychoanalysis, especially in its beginnings, has comparatively de-emphasized both the surface manifestations in their specific identity and, what is more, the so-called distal achievements of behavior. These latter " results " of behavior in turn play the dominant role in such neo-behaviorist systems as that of Tolman. The regrouping of manifest observable facts as undertaken by Freud centers about sameness of " need," that is, sameness of assumed internal cause, while in the case of

Tolman it centers about sameness of effect and in the case of Egon Brunswik's theory of perception it centers about sameness of external object. By virtue of this incompleteness psychoanalysis did not altogether manage to avoid the pitfalls of motivational relativism and of a genetic dissolution of overt adjustmental values. But it must also be stressed that in psychoanalysis we find a legitimate reconstruction of objective causes and not only a pseudo-explanation in terms of subjectively experienced motives as some critics have tried to argue.

It must further be pointed out that the assumption of the dynamisms of the " inner man " to which such behaviorist critics of psychoanalysis as Skinner have objected can be shown to increase the parsimony of the scientific description of behavior patterns. Even more crucial is the fact that these extrapolations from overt behavior help to select the most relevant though often less conspicuous aspects of behavior which otherwise would be lost in the infinite range of possible observation. The relatively great explanatory and predictive value of hypotheses dealing with underlying motivation, which can be demonstrated statistically by means of multiple correlation, may be based on the fact that the selectivity just referred to enters crucially into the formation of these hypotheses. It may be added that from the standpoint of logical analysis there is no alternative but to be behavioristic in any psychological endeavor; neither the so-called subjective phantasies in which psychoanalysis is interested nor any other " introspective " events in others can be constituted except by reference to the manifest physical observation of organisms.

While Feigl places psychoanalysis at the third of the four levels of explanation he distinguishes, thus grouping it together with the relatively descriptive behavior theories of Tolman and Hull, it seems that at least a certain group of psychoanalytic concepts including that of the unconscious goes beyond this level by involving what Reichenbach calls surplus meaning. Using a distinction recently injected into psychological theory by MacCorquodale and Meehl, this group of concepts would seem to be " hypothetical constructs," in contradistinction to the " intervening variables " which are thought of as resting exclusively on the values of a specified set of empirically observed data (Chapter 6). In their own rather sketchy analysis the authors point out that such terms as libido, censorship, or superego were in psychoanalysis originally introduced as intervening variables, that is, as conventionalized designations of observable properties, but that there frequently was an unnoticed shift toward hypothetical constructs. In their arguments the authors tend to overlook the fact that statements containing intervening variables are by no means exhaustible by statements concerning their observational basis. Both Carnap and Hempel have made it explicit that sentences containing disposition terms cannot be fully translated into sentences about observables. Since we cannot specify all conditions and manners in which latent tendencies become manifest, dispositional statements involve " open " terms and require an infinite series of conditions in order to be tested. The distinction between intervening variables and hypothetical constructs may, in our opinion, nonetheless be retained in a modified form as a gradual one involving different degrees of indirectness of evidence, or different kinds of surplus meaning. Possible relationships to the distinction made by Carnap and Hempel between postulatory theoretical constructs and concepts more directly reducible to observation are pointed out. Guided by some relatively fragmentary initial impressions, Freud seems to have proceeded rather directly to the building of a theoretical structure, with empirical interpretation lagging somewhat behind; in the definition of such theoretical constructs as superego, ego and id the major emphasis is on their structural relations to each other rather than on their relations to observation.

In turning to the problems of the objective verification of specific psychoanalytic hypotheses by means of experiments carried out in the formal tradition of psychology proper (Chapters 5, 7, 8), two types of approach may be distinguished. One, and the more customary of the two, takes its start from the particular hypothetical mechanisms for which psychoanalysis has coined its most salient key-terms. In analogy with an observation made by Frank for the field of physics, the more inferential and abstract parts of psychoanalytic theory, such as the hypotheses on repression, projection, and reaction formation, turn out to be less readily accessible to experimental confirmation than the more descriptive ones, such as those on fixation and regression. Some of the experimental studies have verified even such seemingly far-fetched psychoanalytic assumptions as symbolism.

A second type of approach, one which the present writer has tried to develop for a number of years, concentrates on the principle of alternative manifestations of motivational tendencies. This principle describes the basic pattern of interrelationships between the two strata involved in all psychoanalytic theory, the manifest and the latent, and can be shown to underlie most if not all of the specific mechanisms just mentioned. The possibility of analyzing statistically the tangled relationships between the two strata after imbuing them with some degree of operational independence is illustrated by studies dealing with motivation in its relation to overt behavior segments and with certain mechanisms of self-deception. In the former study a comparison of overall motivational ratings with specific behavioral manifestations is used for a " rational reconstruction " of the cues underlying the so-called intuitive inferences made by the clinicians; the same general procedure would apply in case of the more explicit and more scrutinizing inferences concerning motivational dynamics made by the psychologist as a scientist rather than as a synoptic rater. In the study of self-deception certain formal criteria of distortion, which may take their place alongside the more content-oriented type of diagnostic criteria favored in psychoanalysis proper, were established by means of a semantic analysis of the individuals' responses.

The verification of psychoanalytic hypotheses rests only to a small part on the systematic evidence furnished by academic psychology. Psychoanalysis itself has provided confirmatory empirical data of overwhelming scope, ranging from the wealth of evidence given by individual patients to a synopsis of dream mechanisms, of lapses of tongue and of memory, of pathological symptoms, and of certain relevant features of folklore, myth, and other cultural phenomena.

Regardless of how imperfect psychoanalytic theory may be as to its formal structure, it has no rival among psychological theories as far as the range of both its evidence and its explanatory power is concerned. Learning theory, which is the only other serious contender in such a contest, is equally ambitious in its claim to universality and at the same time more formalized than is psychoanalytic theory. It shares with psychoanalysis the " historical " outlook as well as some basic explanatory principles. But it falls short in accounting for such crucial facts as neurotic inaccessibility to " reinforced " experience which psychoanalysis has so impressively demonstrated and explained, or for the progressive strengthening of the superego in spite of the absence of appropriate reinforcement.

An effort is further made to relate the motivational aspects dealt with in psychoanalysis to certain cognitive problems (Chapter 9). Acceptance of the ambiguous relationship between motivation and manifestation which is the chief discovery of psychoanalysis requires cognitive " tolerance of ambiguity " on the part of the scientist. Relations to the principle of the closeness of opposites, and to the analysis of concretistic, compulsive, and dogmatic patterns of perception and thought by such orthodox psychoanalysts as Fenichel and Reik, are pointed out. The necessary complementarity of the study of motivation and of cognition within a fullfledged system of psychology is elaborated on in its relationship to the complementarity of the depth and the surface approach within the psychology of motivation itself. A more formal aspect of the interrelatedness of the psychology of motivation and of cognition is given by the semantic difficulties arising from the employment of a partly identical nomenclature for the latent and the manifest stratum in common-sense language; as pointed out earlier in the paper, these difficulties are analogous to those arising in the psychology of perception in consequence of the confusion of stimulus and response terminologies as inherited from traditional philosophy.

A concluding section (Chapter 10) deals with the ethically constructive aspects of psychoanalysis. Psychoanalysis combines the genetic reduction of infantile conscience to irrational patterns with a stress on personal maturity and rationality as the adult ethical ideal. Some applications to the problem of the acceptance of scientific theories are made.

REFERENCES

1. Allport, G. W., *Personality*. New York: Holt, 1937.
2. Barker, R., Dembo, T., and Lewin, K., Frustration and regression: an experiment with young children. *Univ. of Iowa Studies in Child Welfare*, 1941, *18*, No. 1.
3. Bergmann, G., Psychoanalysis and experimental psychology. *Mind*, 1943, *52*, 122-140.

4. Bergmann, G., Theoretical psychology. *Annual Review of Psychol.*, 1953, *4*, 435-458.

5. Bernfeld, S., Freud's earliest theories and the school of Helmholtz. *Psychoanal. Quart.*, 1944, *13*, 341-362.

6. Bernfeld, Suzanne C., Freud and archeology. *Amer. Imago*, 1951, *8*.

7. Betlheim, S., and Hartmann, H., Ueber Fehlreaktionen bei der Korsakowschen Psychose. *Arch. Psychiatr. Nervenkrank.*, 1924, *72*, 275-286.

8. Bleuler, E., *Dementia Praecox or the Group of Schizophrenias.* Transl. by J. Zikin, New York: Internat. Universities Press, 1950.

9. Blum, G. S., *A study of the psychoanalytic theory of psycho-sexual development.* Genet. Psychol. Monog., 1949.

10. Brenman, M., Studies of normal and hypnotic recall of fairy tales heard in childhood. (Unpublished paper as referred to in *Emotions and Memory*, ed. by D. Rapaport, Menninger Clinic Monogr. Series, Vol. 2, 1942.)

11. Bridgman, P. W., *The Logic of Modern Physics.* New York: Macmillan, 1928.

12. Brunswik, E., *Wahrnehmung und Gegenstandswelt.* Vienna: Deuticke, 1934.

13. ———, *The Conceptual Framework of Psychology.* Internat. Encycl. of Unified Science, Vol. I, No. 10. Chicago: Univ. of Chicago Press, 1952.

14. Cameron, N., *The Psychology of Behavior Disorders.* New York: Houghton-Mifflin, 1947.

15. Carnap, R., Testability and Meaning. *Philos. of Science*, 1936, *3*, and 1937, *4*.

16. ———, Logical foundations of the unity of science. In *Encyclopedia and Unified Science*, Internat. Encycl. of Unified Science, Vol. I, No. 1, University of Chicago Press, 1938.

17. Dingle, H., The logical status of psychoanalysis. *Analysis*, 1949, *9*, 63-66.

18. Dollard, J., and Miller, N. E., *Personality and Psychotherapy.* New York: McGraw-Hill, 1950.

19. Durkheim, E., *The Rules of Sociological Method.* Translation, Glencoe: Free Press, 1950.

20. Einstein, A., Remarks on Bertrand Russell's theory of knowledge. In: *The Philosophy of Bertrand Russell*, Library of Living Philosophers, ed. by P. A. Schilpp, Vol. 5. Evanston: Northwestern Univ. Press, 1944, p. 289.

21. Ellis A., *An Introduction to the Principles of Scientific Psychoanalysis.* Genet. Psychol. Monographs, 1950, Vol. 41, 147-212.

22. Feigl, H., Some remarks on the meaning of scientific explanation. In *Readings in Philos. Analysis*, ed. by H. Feigl and W. Sellars, New York: Appleton-Century-Crofts, 1949.

23. ———, Existential hypotheses. *Philosophy of Science*, 1950, *17*.

24. Fenichel, O., *Psychoanalytic Theory of Neurosis.* New York: Norton, 1945.

25. Flew, A., Psychoanalytic explanation. *Analysis,* 1949, *10,* 8-15.

26. Frank, P., *Modern Science and Its Philosophy.* Cambridge, Mass.: Harvard Univ. Press, 1941.

27. Frenkel-Brunswik, E., Mechanisms of self-deception. *Jour. of Social Psychol.,* 1939, *10,* 409-420.

28. ————, Psychoanalysis and personality research. *Jour. of Abnormal and Social Psychology,* 1940, *35,* 176-197. (In: *Symposium on Psychoanalysis by Analyzed Psychologists,* reprinted Washington, D. C.: Amer. Psychol. Assn., 1953.)

29. ————, *Motivation and Behavior.* Genetic Psychol. Monog., 1942.

30. ————, Intolerance of ambiguity as an emotional and cognitive personality variable. *Jour. of Personality,* 1949, *18,* 108-143.

31. Freud, A., *The Ego and the Mechanisms of Defense.* London: Hogarth, 1937.

32. Freud, S., Character and anal erotism (1908). *Coll. Papers, 2,* 45-50. London: Hogarth, 1924.

33. ————, A note on the unconscious in psychoanalysis (1912). *Coll. Papers, 4,* 22-29. London: Hogarth, 1925.

34. ————, Instincts and their vicissitudes (1915). *Coll. Papers, 4,* 60-83. London: Hogarth, 1925.

35. ————, The unconscious (1915). *Coll. Papers, 4,* 98-136. London, Hogarth, 1925.

36. ————, My views on the part played by sexuality in the aetiology of the neuroses (1915). *Coll. Papers, 1,* 277. London: Hogarth, 1940.

37. ————, *Beyond the Pleasure Principle* (1920). In *General Selection from the Works of Sigmund Freud,* ed. by J. Rickman, London: Hogarth, 1937.

38. ————, *The Ego and the Id* (1923). In *General Selection from the Works of Sigmund Freud,* ed. by J. Rickman, London: Hogarth, 1937.

39. ————, *New Introductory Lectures on Psychoanalysis.* New York: Norton, 1933.

40. ————, *An Autobiographical Study* (1924). Internat. Psychoanal. Lib., 26.

41. ————, *An Outline of Psychoanalysis* (1940). Transl. by J. Strachey, New York: Norton, 1949.

42. Goldstein, K., and Scheerer, M., *Abstract and concrete behavior.* Psychol. Monog., No. 239, 1941.

43. Hartmann, H., Ichpsychologie und Anpassungsproblem. *Internat. Zeitschrift für Psychoanalyse und Imago,* 1939, *24,* 62-135.

44. Hempel, C. G., *Fundamentals of Concept Formation in Empirical Science.* Internat. Encycl. of Unified Science, Vol. II, No. 7. Chicago: Univ. of Chicago Press, 1952.

45. Hilgard, E. R., Experimental approaches to psychoanalysis. In *Psychoanalysis as Science,* ed. by E. Pumpian-Mindlin, Stanford University Press, 1952.

46. Hunt, J. McV., The effects of infant feeding-frustration upon adult hoarding in the albino rat. *Jour. Abnorm. and Social Psychol.,* 1941, *36,* 338-360.

47. Jaensch, E. R., *Der Gegentypus.* Leipzig: Barth, 1938.

48. Kretschmer, E., *Körperbau und Charakter.* Berlin: Springer, 1922; 20th ed., 1951.

49. Kubie, L. S., Problems and techniques of psychoanalytic validation and progress. In *Psychoanalysis as Science,* ed. by E. Pumpian-Mindlin, Stanford University Press, 1952.

50. MacCorquodale, K., and Meehl, P., On a distinction between hypothetical constructs and intervening variables. *Psychol. Rev.,* 1948, *55,* 95-107.

51. Maier, N. R. F., *Frustration: The Study of Behavior Without a Goal.* New York: McGraw-Hill, 1949.

52. Masserman, J. H., *Behavior and Neurosis.* Chicago: University of Chicago Press, 1943.

53. Mowrer, O. H., *Learning Theory and Personality Dynamics.* New York: Ronald, 1950.

54. Murray, H. A., and workers at Harvard Psychol. Clinic, *Explorations in Personality.* New York: Oxford Univ. Press, 1938.

55. Neurath, O., Unified science as encyclopedic integration. In *Encyclopedia and Unified Science,* Internat. Encycl. of Unified Science, Vol. I, No. 1. Chicago: University of Chicago Press, 1938.

56. Piaget, J., *The Child's Conception of the World.* Translation, 1929.

57. Reichenbach, H., *Experience and Prediction.* Chicago: Univ. of Chicago Press, 1938.

58. Reik, T., *Masochism in Modern Man.* New York: Farrar, 1941.

59. ————, *Dogma and Compulsion.* New York: Internat. Universities Press, 1951.

60. Schlick, M., *Raum und Zeit in der gegenwärtigen Physik.* 3rd ed., 1920. Transl., New York: Oxford Univ. Press, 1920.

61. ————, *Allgemeine Erkenntnislehre,* Berlin: Springer, 1918; 2nd ed., 1925.

62. Schroetter, K., Experimentelle Traeume. *Zentralblatt für Psychoanal.,* 1936, 7, 151-163.

63. Sears, R. R., Experimental studies of projection: I. *Jour. Social Psychol.,* 1936, 7, 151-163.

64. Sears, R. R., *Survey of Objective Studies of Psychoanalytic Concepts.* Soc. Science Research Council Monog., 1943, No. 51.

65. Shoben, E. J., A learning theory interpretation of psychotherapy. *Harvard Educ. Rev.*, 1948, *18*, 129-145.

66. Skinner, B. F., *Science and Human Behavior.* New York: Macmillan, 1953.

67. Stevens, S. S., Psychology and the science of science. *Psychol. Bull.*, 1939, *36*, 221-263.

68. Tolman, E. C., *Purposive Behavior in Animals and Men.* New York: Appleton-Century, 1932.

69. Toulmin, S., The logical status of psychoanalysis. *Analysis*, 1948, *9*, 23-29.

70. Weber, Max, *The Protestant Ethic and the Spirit of Capitalism.* Transl. by T. Parsons, New York: Scribner's, 1930.

71. Weber, Marianne, *Max Weber, Ein Lebensbild.* Heidelberg: Lambert-Schneider, 1950.

72. Werner, H., *Comparative Psychology of Mental Development.* Rev. ed., Chicago: Follett, 1948.

73. Wolf, A., The dynamics of the selective inhibition of specific functions in neurosis: a preliminary report. *Psychosom. Med.*, 1943, *5*.

INDEX TO VOLUME 80

Barber, Bernard, and Merton, Robert K., *Brief Bibliography for the Sociology of Science*, 140

Barrett, F. and Shepard, Herbert A., *A Bibliography of Cybernetics*, 204

Bergmann, Gustav, *Comments on Professor Hempel's "The Concept of Cognitive Significance,"* 78

Bibliography of Cybernetics, 204

Black, Max, *Comments on Preceding Paper of W. V. Quine*, 97

Brief Bibliography for the Sociology of Science, 140

Brief Bibliography of Formal Logic, 155

Carnap, Rudolf, *Inductive Logic and Science*, 189

Church, Alonzo, *Brief Bibliography of Formal Logic*, 155

  The Need for Abstract Entities in Semantic Analysis, 100

Comments on Preceding Paper of W. V. Quine, 97

Comments on Professor Hempel's "The Concept of Cognitive Significance," 178

Conant, James B., *Greetings*, 9

Concept of Cognitive Significance: A Reconsideration, 61

Frank, Philipp, *Introductory Remarks*, 5

  The Logical and Sociological Aspects of Science, 16

  The Origin of the Separation Between Science and Philosophy, 115

Greetings, 9

Hempel, Carl G., *The Concept of Cognitive Significance: A Reconsideration*, 61

Inductive Logic and Science, 189

Integration in the Biological and Sociological Sciences, 31

Introductory Remarks, 5

Logical and Sociological Aspects of Science, 16

Lotz, John, *Natural and Scientific Language*, 87

Merton, Robert K. (See Barber, Bernard, and Merton, Robert K.)

Morris, Charles, *The Science of Man and Unified Science*, 37

Natural and Scientific Language, 87

Need for Abstract Entities in Semantic Analysis, 100

On Mental Entities, 198

Origin of the Separation Between Science and Philosophy, 115

Quine, W. V., *Semantics and Abstract Objects*, 90

  On Mental Entities, 198

Reichenbach, Hans, *The Verifiability Theory of Meaning*, 46

Science of Man and Unified Science, 37

Semantics and Abstract Objects, 90

Sheldon, W. H., *Integration in the Biological and Sociological Sciences*, 31

Shepard, Herbert A. (See Barrett, F. and Shepard, Herbert A.)

Significs, Its Tendency, Methodology and Applications, 223

Some Significant Trends Toward Integration in the Sciences of Man, 173

Thompson, Laura, *Some Significant Trends Toward Integration in the Sciences of Man*, 173

Verifiability Theory of Meaning, 46

Vuysje, D., *Significs, Its Tendency, Methodology and Applications*, 223

Rumford Medal Lecture

American Academy of Arts and Sciences
1951

Adventures With Standing Light Waves

HERBERT E. IVES

Vol. 81, No. 1
Proceedings of the American Academy of Arts and Sciences

Vol. 81, No. 1, The Rumford Medal Lecture, pp. 1-32,
November, 1951, $0.75

THE RUMFORD MEDAL LECTURE

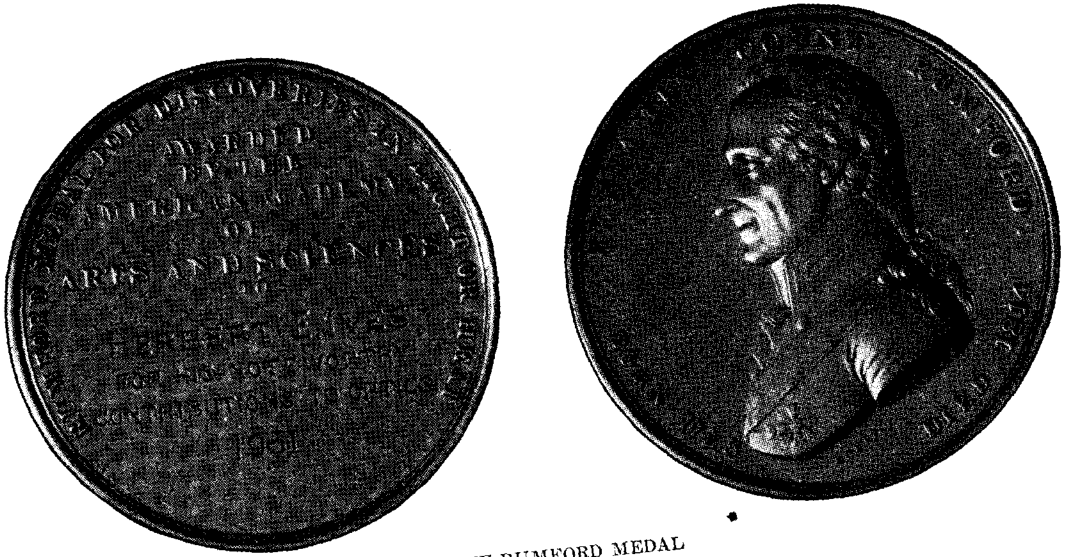
The Rumford Medals are awarded by the American Academy of Arts and Sciences to the outstanding American contributors to the sciences of heat and light. This biennial premium was established in 1796 by a gift of the American-born scientist, Benjamin Thompson, Count Rumford, who won lasting fame by his contributions to the establishment of the energy theory of heat and other work in heat and light.

It has become the custom for the recipient of the Rumford Premium to present a communication to the Academy at the time he is awarded the gold and silver medals. In recent years the medalist has reviewed significant aspects of his own work. Believing that the publication of these communications would constitute a noteworthy record of American contributions to heat and light, the Academy inaugurates with this issue of its Proceedings the first of a proposed series of biennial Rumford Medal Lectures.

Harlow Shapley, *Chairman*
Rumford Committee

RECIPIENTS OF THE RUMFORD PREMIUM

1839	Robert Hare
1862	John Ericsson
1865	Daniel Treadwell
1866	Alvan Clark
1869	George Henry Corliss
1871	Joseph Harrison, Jr.
1873	Lewis Morris Rutherford
1875	John William Draper
1880	Josiah Willard Gibbs
1883	Henry Augustus Rowland
1886	Samuel Pierpont Langley
1888	Albert Abraham Michelson
1891	Edward Charles Pickering
1895	Thomas Alva Edison
1898	James Edward Keeler
1899	Charles Francis Brush
1900	Carl Barus
1901	Elihu Thomson
1902	George Ellery Hale
1904	Ernest Fox Nichols
1907	Edward Goodrich Acheson
1909	Robert Williams Wood
1910	Charles Gordon Curtis
1911	James Mason Crafts
1912	Frederic Eugene Ives
1913	Joel Stebbins
1914	William David Coolidge
1915	Charles Greeley Abbot
1917	Percy Williams Bridgman
1918	Theodore Lyman
1920	Irving Langmuir
1925	Henry Norris Russell
1926	Arthur Holly Compton
1928	Edward Leamington Nichols
1930	John Stanley Plaskett
1931	Karl Taylor Compton
1933	Harlow Shapley
1937	William Weber Coblentz
1939	George Russell Harrison
1941	Vladimir Kosma Zworykin
1943	Charles Edward Kenneth Mees
1945	Edwin Herbert Land
1947	Edmund Newton Harvey
1949	Ira Sprague Bowen
1951	Herbert E. Ives


AWARDED
BY THE
OF
ARTS AND SCIENCES
OR HEAT

THE RUMFORD MEDAL

Rumford Medal Lecture 1951

ADVENTURES WITH STANDING LIGHT WAVES

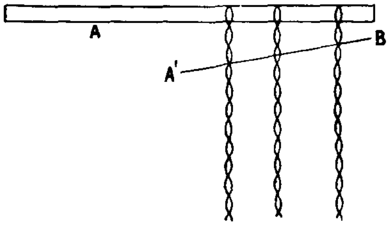
Herbert E. Ives

To me, the most interesting form of literature, far excelling detective stories, is scientific biography. Here one learns how the master followed up the clues, why he chose one fork of the road instead of another, why he picked one mode of investigation or instrumentation where another investigator had picked a less successful one. In this reading of scientific biography and original sources, one cannot fail to be struck by the phenomenon of *recurrence:* the scientist does not clean up the problem on the first attack, he goes back after an interval to try a different fork in the road, perhaps made more promising by other advances in the field. We find such men as Maxwell and Rayleigh going back to the same problems of color measurement after long intervals of working on other things. Another example of recurrence is shown by a scientist using the same scientific tool, in which he has become adept, to attack problems in different fields. This was impressed upon me by an early experience in the Bell Telephone Laboratories, on the occasion of an eclipse of the sun occurring in the New York Area, when a group of telephone engineers were much excited over the possibility of assembling gadgets so that they might *listen* to this gorgeous spectacle!

In casting about for a topic on which to address you tonight, I noticed that my recurrent preoccupation, either as a subject for study and experiment, or as a tool, has been the phenomenon of standing waves of light. It was the subject of my doctor's dissertation at Johns Hopkins, forty-some years ago, and I find myself still speculating on it, with the paper and pencil, which are the laboratory of a retired experimental physicist. I would like to tell you something of my adventures with these standing waves over the years.

The phenomenon of the interference of light waves, elucidated by Young and Fresnel, familiarized the scientific world with the fact that two beams of light, in constant phase relation, could be superposed to produce regions of darkness. The most extreme case of this is offered by the superposition of two beams proceeding in the same straight line but in opposite directions. According to wave theory, whether the waves be tensional waves along a string, or waves of sound in air, or waves of light, the region above a reflecting surface should exhibit layers in continuous agitation, between which are regions of no activity. Such systems are called standing waves.

The existence of standing waves of light was not demonstrated until nearly a century after the convincing experiment of Fresnel on bright spots being formed in the middle of shadows. In 1890 Otto Wiener performed the experiment which is illustrated by Slide 1. He directed a beam of light perpendicularly upon a mirror. Above the mirror he placed an exceedingly thin photographic film, at a small angle to the surface. As the diagram shows, this film should pass through regions where the standing waves were forming nodes and loops. At the nodes there should be no photographic action, at the loops, strong action. This was in fact what he found, by the bands of photographic deposit along the developed film. Here was a demonstration of the wave nature of light, more convincing even than the classical experiments of Fresnel; for while corpuscles could be imagined as deviated into or out of a shadow, here corpuscles, if they existed, would have to be imagined as passing out of existence and being reborn, as they went to and from the mirror. Waves of course present no comparable difficulty.



SLIDE 1

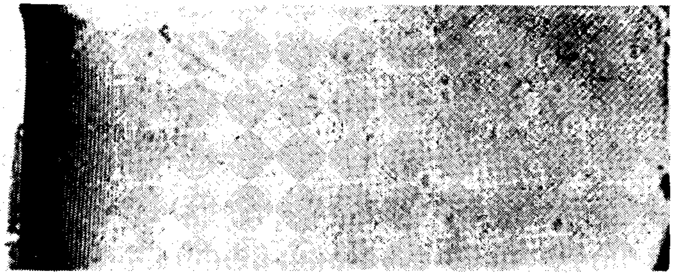
The next contribution to this subject which I shall notice was actually made several years before Wiener performed his experiment. In a paper by Lord Rayleigh in 1887, on *The Maintenance of Vibrations by Forces of Double Frequency, and on the Propagation of Waves through a Medium Endowed with a Periodic Structure,* this footnote appears: "A detailed experimental examination of various cases in which a laminated structure leads to a powerful but highly selected reflection would be of value. It has occurred to me that Becquerel's reproduction of the spectrum in natural colours upon silver plates may perhaps be explicable in this manner. The various parts of the film of subchloride of silver with which the metal is coated may be conceived to be subjected, during exposure, to *stationary* luminous waves of nearly definite wave-length, the effect of which might be to impress upon the substance a periodic structure recurring at intervals equal to half the wave-length of the light; just as a sensitive flame exposed to stationary sonorous waves is influenced at the loops but not at the nodes.'' (Here he gives a reference to an observation of his own eight years before.) ''In this way the operation of any kind of light would be to produce just such a modification of the film as would cause it to reflect copiously that particular kind of light. I abstain at present from developing this suggestion, in the hope of soon finding an opportunity of making myself experimentally acquainted with the subject.''

However, before Lord Rayleigh recurred to the subject of standing waves—he went off on another tack and discovered argon—this particular problem was taken up by Gabriel Lippmann, who actually made photographs in color along the lines of Rayleigh's suggestion and received the Nobel Prize for his work. Lippmann's procedure, in brief, was to place a photographic plate, coated with a very transparent photographic emulsion, in contact with a mirror of mercury, so that standing waves were formed in the thickness of the emulsion. When developed, the plate showed, on examination by reflected light, colors, which in the case of the bright pure colors of the spectrum, were closely those of the originally incident light. I show such a photograph of the spectrum, which, from the date recorded on it, I find I made in April 1907.

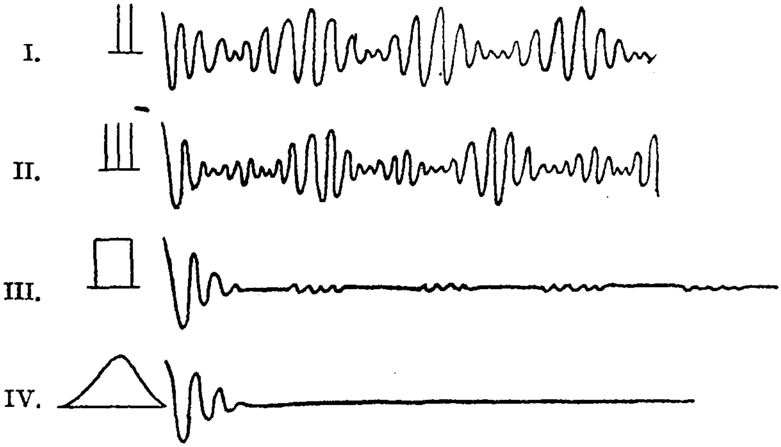
This process was my introduction to the field of standing waves. There were some things not clear about these Lippmann pictures. The reproduction of colors, while striking, was not accurate. Monochromatic radiations were reproduced as broad spectrum bands; mixed colors were a challenge poorly met; it was questioned in some quarters whether the complicated laminated structure demanded by simple theory really existed. This was long before the days of the ultraviolet microscope and of course of X-ray or electron diffraction analysis.

Having been brought up in an atmosphere of color photography, this was a problem that excited my interest, and I chose a study of the Lippmann process for my doctor's dissertation at Johns Hopkins. Fate played into my hands, for just at that time Ramón y Cajal reported the observation that the structure of Lippmann films could be enlarged to observable dimensions by the simple expedient of soaking them in water, whereby they were expanded many times and brought into the range of ordinary microscopic observation.

Let me show you by a series of slides what this technique of observation brought out with regard to the actual nature of the Lippmann photographs. Slide 2 shows a section of a water-swollen section of a Lippmann film, made by exposure to monochromatic light. You will see that the layers of photographic deposit actually exist; but you will see that there are only a very few of them, close to the surface, in agreement with the observation that the reflected colors are far from pure. This point is brought out by Slide 3, which shows the calculated



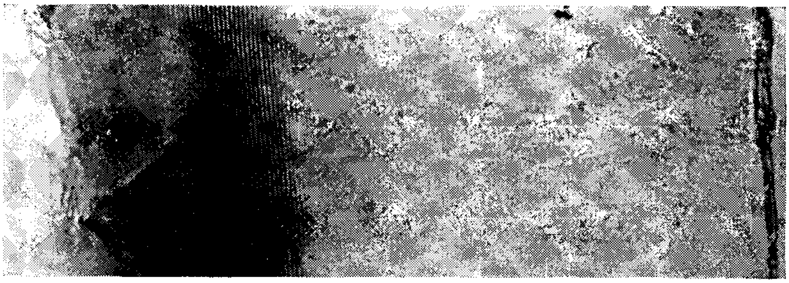
SLIDE 2



SLIDE 3

standing wave systems for various monochromatic and mixed radiations. The thin layer of photographic action shown in the first photograph is obviously incapable of giving an accurate reproduction of all of these.

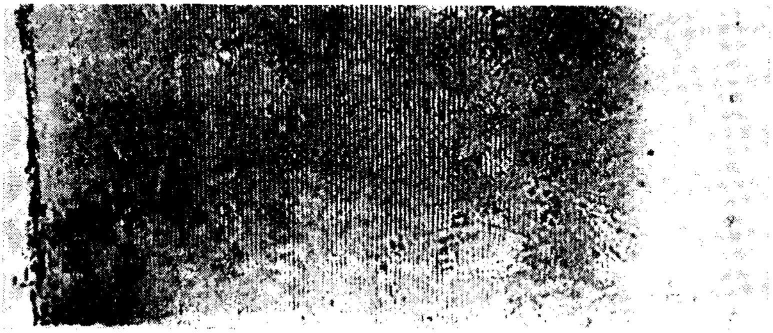
Some knowledge of photographic processes, acquired in a home where a photographic darkroom was considered quite as necessary as a kitchen, gave a hint to a productive line of inquiry. Lippmann plates had heretofore been developed with a very quick acting developer, namely pyrogallol, which had to be stopped in its action before the plate fogged. Now there were other developers whose action was so slow that the whole thickness of the photographic emulsion was wet before the developing action was appreciable. One such developer was hydrochinon. The next step was to develop a Lippmann plate with this reagent. Slide 4 shows a section of the resultant film.



SLIDE 4

You will see that the laminations extend to a much greater depth, but cease abruptly. Why should the standing waves only exist to a certain depth? The answer to this was not far to seek. The films thus far used had been made color sensitive by the common process of bathing with sensitizing dye. It was a likely guess that it was the sensitizer, rather than the standing waves, that had penetrated only a short distance. The next step was to color sensitize by putting the dye in the emulsion before the plate was flowed. Slide 5 shows a section of such a plate after exposure to monochromatic light and development by hydrochinon. You will see that we now have laminae clear through the film, competent to produce light by reflection of a high degree of spectral purity.

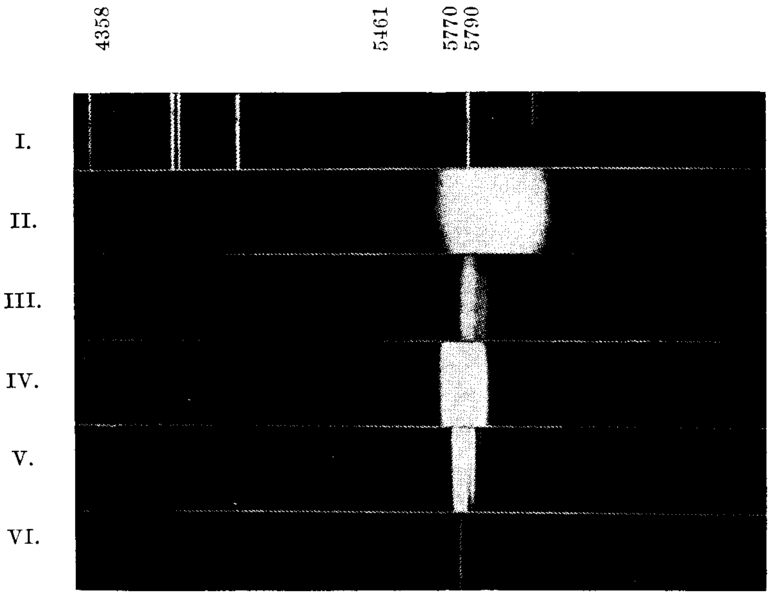
There remained one further step to be taken. While the laminae were thus proved to be present throughout the film they were useless for producing monochromatic light, because they were opaque, and only a few top laminae were effective. Here again a familiarity with photographic processes came to the rescue, in this case the observation that the first step in intensification with mercuric chloride changed a black photographic deposit into a transparent one. The plates with their black opaque laminations were therefore treated with mercuric chloride, and there resulted laminations of alternating higher and lower refractive index, transparent to light from the whole depth of the film.



SLIDE 5

The performance of Lippmann plates so developed and so treated is shown in Slide 6, where the rendering of monochromatic radiation (the green mercury line) by films of various thicknesses is exhibited. The light reflected from the thickest film, shown at the bottom, is, in the spectrogram, hardly distinguishable from that rendered from the mercury arc directly by the spectrograph. It will be seen from these that the study was successful in clearing up the doubts which had troubled some, as to the colors being true standing wave products, and also had evolved processes for making the method approach its theoretical possibilities.

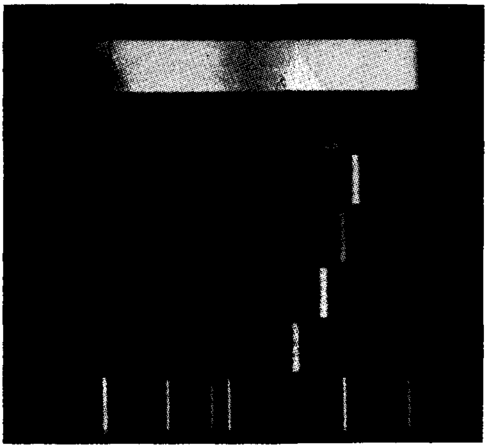
In the years immediately following this work I was occupied with problems of photometry and illumination. That was a heroic period in photometry, with new, more efficient light sources being rapidly developed, with a host of problems connected with their varying colors, and the need for correlating photometry with the newly formulated laws of radiation. Illustrative of my introductory remarks, I found a use for standing waves in one of the photometric problems. One of the older suggestions for comparing lights of different colors was what was known as the Crova method. This consisted in making the observation by monochromatic light. It suffered from the disadvantage that one must use a spectroscope to get the monochromatic light, and that the exact wave-length to be used had to be slightly varied according to the emission characteristics of various sources. Now these special Lippmann plates offered a simple means for observing by monochromatic light, and they had the peculiarly apt characteristic of shifting



SLIDE 6

the wave-length of the reflected light by a slight change of the angle at which they were observed, — they were "light sirens". In Slide 7 I show a spectrogram, made to illustrate these properties, in which you see how a highly monochromatic reflection is shifted along the spectrum by rotation of the plate. This method of photometry was not destined to have wide use, because the newer illuminants presented too much diversity of radiating characteristics for the Crova method to be valid. I tell you about it merely to illustrate how an investigator recurs to tools with which he is familiar.

All work along these lines was interrupted when we entered the war in 1917, and I found myself in the Army Air Service, working on apparatus and methods for aerial photography. The technical problems were sufficiently engrossing, but hardly less difficult was that of convincing the military mind of the value of photographic reconnaissance. According to Sir Douglas Haig, "reconnaissance always has been and always will be made by cavalry". Now cavalry has disappeared, and millions of aerial photographs were made in the last war, by apparatus and methods many of which had their inception in our Langley Field laboratory.



SLIDE 7

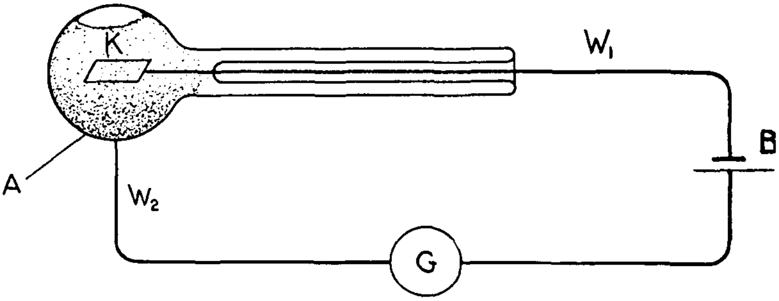
Following the war I went to the Bell Telephone Laboratories. When asked what I would choose to work on I said "the photoelectric effect" with which I had become enamored as a tool for photometry. I was gently but firmly told that no use was foreseen for the photoelectric effect, and instead I was to work on a very pressing problem, namely electrical contacts. This I did for several years, with results of some value in connection with the reliability of the telephone, but of no interest to the Rumford Medal Committee. I did use optical methods at times in the study of contact operation, but not standing waves.

I must confess that while working on electrical contacts I was insubordinate to the extent of doing a little work on the side with photoelectricity. This did not prove to be such a bad thing, because it gave me confidence to urge that the problem of transmitting pictures over telephone lines be seriously studied. The outcome of this was the picture transmitting system which has now become nationwide and so commonplace as to be taken for granted. Thirty years ago, as some of you will remember, when I point it out, newspaper pictures of distant events appeared only days afterward. Now it is usual for the majority of newspaper illustrations to come in by wire, as quickly from San Francisco as from Cambridge.

As a sequel to the transmission of photographs came the first complete realization of television, with the transmission of moving images beyond the limits set by the curvature of the earth. This development, a combination of the photoelectric cell, tracing back to Hertz and Elster and Geitel, with the audion of DeForest, has gone far since 1927 when in New York we talked with and saw people in Washington; and the end is not yet in sight.

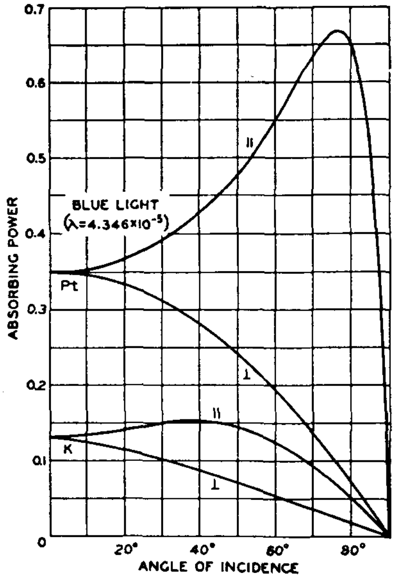
We seem to have wandered rather far from the subject of standing waves, but they are to come up again. After this intensive period of application to the problem of putting vision into the communication art, when I again raised the question of studying the photoelectric effect, there was no voice raised to protest that it was an unpromising field. It was in the studies then started that I found myself once again playing with standing light waves.

These studies were aimed at elucidating certain problems in the nature of the photoelectric effect; finding "the go of it" as Maxwell put it. Why are photoelectrons more copiously emitted at certain wave-lengths than others; why is the emission enormously affected by the state of polarization of the exciting light? In order to make clear what these problems were, let us look at a schematic photoelectric cell of the type in which these phenomena are most strikingly exhibited. In Slide 8 we have depicted a glass bulb, in the center of which is a flat highly polished metal plate, say of platinum. The inside of the bulb is given a conducting metallic coating, with a window through which light may enter. This coating, and the central plate, are both connected, by wires through the glass, with an external closed circuit, furnished with current measuring instruments. Into the bulb is introduced a small quantity of an alkali metal such as potassium. The bulb is exhausted to a high vacuum, the metal plate is raised to a high temperature by thermionic bombardment, to clean it, and the alkali metal allowed to build up, by its vapor pressure, an extremely thin stable film, only a few atoms thick, so that the metal plate becomes a photosensitive cathode.



Slide 8

Let us consider first what happens when the light falling on the cathode is polarized. It has long been known that at high angles of incidence the emission is greater for light polarized with the electric vector cutting into the surface. This was in agreement with the fact that the absorption of light is then greater, but the puzzling fact was encountered that this difference in emission for the two planes of polarization, which could only amount to a factor of two or three to one, on the basis of the absorption, actually amounted in the case of these thin films to ten or more. This is illustrated by the next slides,

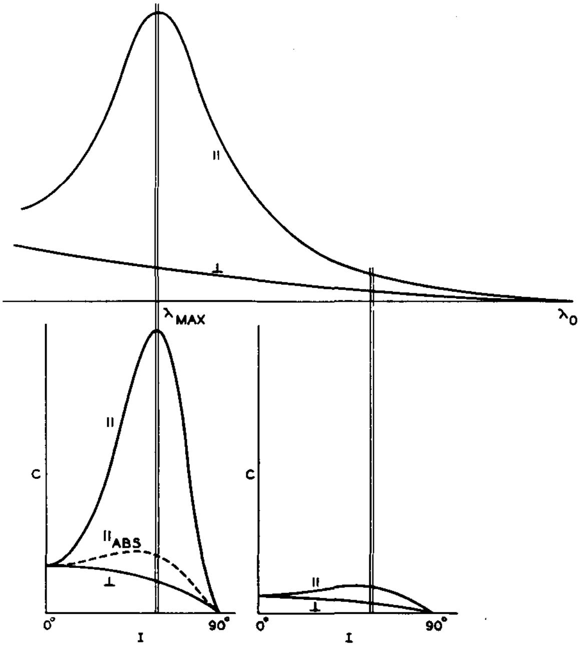


Slide 9

where we have first in Slide 9 the absorption of light for platinum and potassium at different angles of incidence, for the two planes of polarization (denoted by || and ⊥), showing a maximum difference at 60 degrees incidence represented by a factor of about two; and in Slide 10 (lower left diagram) the photoelectric emission from a thin film of potassium on platinum for the same conditions of illumination; here we see the ratio of emissions at 60 degrees amounting to nearly ten times.

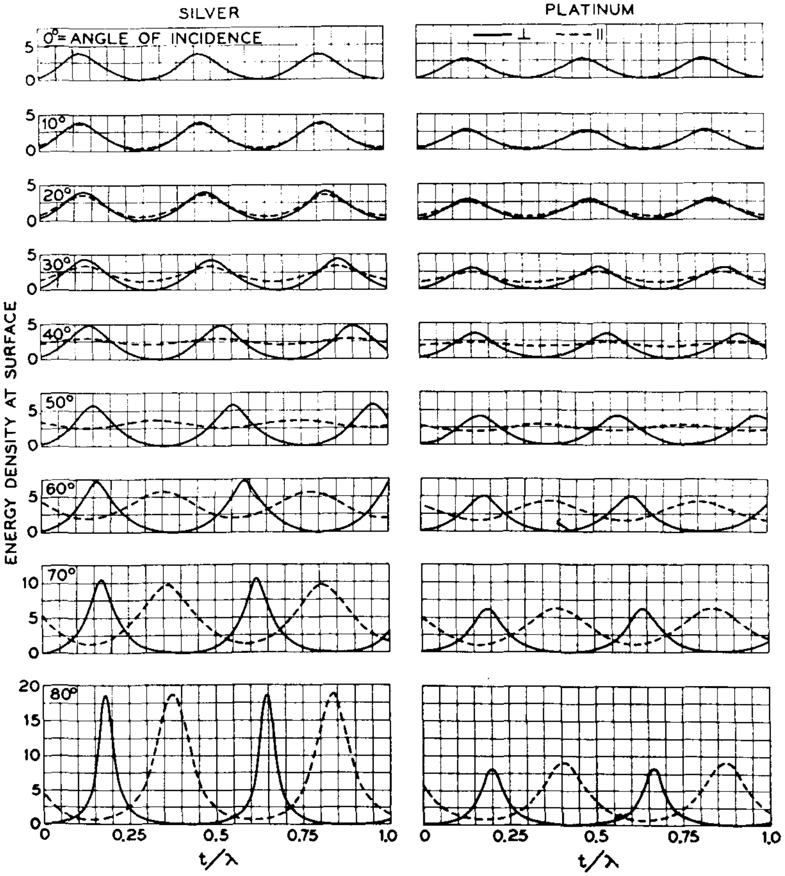
The clue to this phenomenon is found when we recognize that above the metal mirror there exists a system of *standing waves.* The film of potassium, of thickness only a small fraction of a wave-length, lies just above the mirror surface; the light energy active in it will be that of the standing wave system at that point. This can be calculated from the refractive index and extinction coefficient of the metal base. In Slide 11 we see these calculated standing waves from the surface outward for two representative metals, silver and platinum, for the two planes of polarization, for light incident at various angles from zero to 80 degrees. You will note that while for normal incidence the standing waves for both planes of polarization exhibit a node at the surface, at higher angles of incidence the plane for which the electric vector cuts into the surface approaches a loop. The consequence of this is clearly shown in Slide 12, where the computed intensity curves, at various angles of incidence, are shown (full lines) for the two planes, || and ⊥.

You will see that the crucial point — the enormous ratio of the photoelectric emission for the two planes of polarization at high angles of incidence — is completely accounted for by this theory that the emission is proportional to the energy density in the standing wave system set up over the supporting metal surface. The success of the theory even to small details is shown by Slide 13, where the photoelectric emission of a thin film of rubidium on glass is shown, as computed (full line) and as measured (dots and crosses). Notice that in this case the emissions for both planes of polarization increase toward a maximum and then decline, a behavior previously not observed, but predicted from this theory, and accurately matched on experimental search.



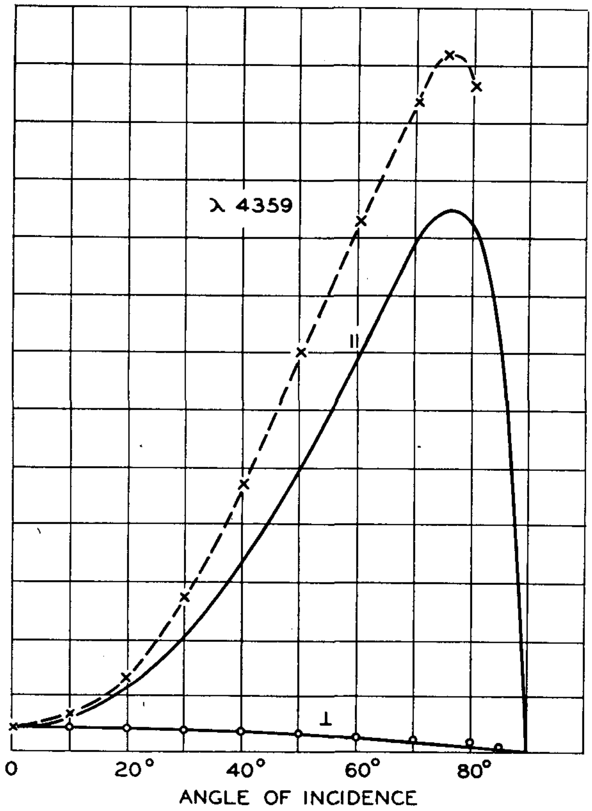
Slide 10

The second problem presented by the photoelectric emission from alkali metals is the spectral distribution. It has been known for a long time that the maxima of emission move progressively toward the red with increasing atomic weight, as shown by Slide 14. The reason for this was completely vague; some attempts had been made to correlate the maxima of emission with the size of electronic orbits. No relationship with the optical properties of the materials had been explored, partly due to the fact that the optical properties of the alkali metals were imperfectly known.



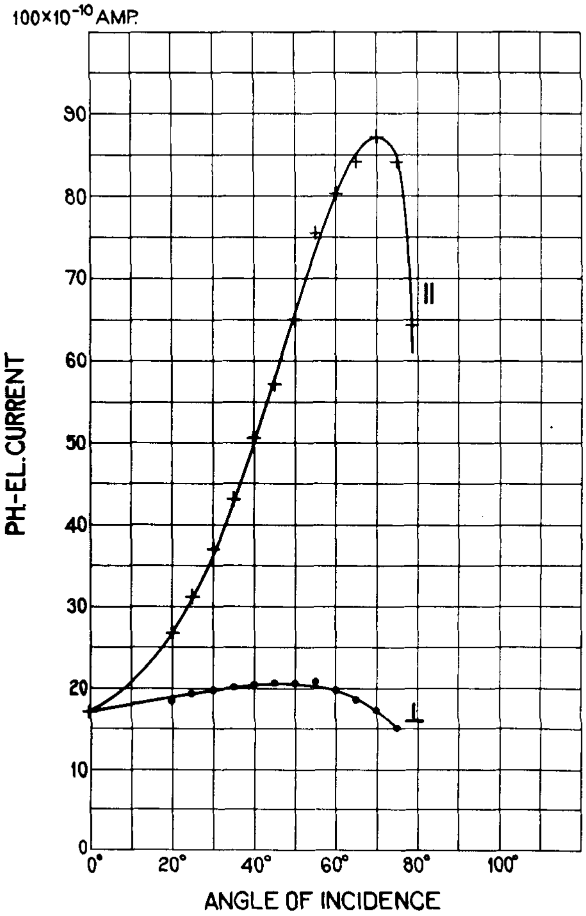
Slide 11

The same line of attack which you have seen lead to the elucidation of the vectorial and polarization phenomena has led to a very complete understanding of the spectral emission characteristics of thin photoelectric films. The picture to which we are led is that the thin film at the foot of the standing wave system absorbs light according to the intensity of the standing wave, and in proportion to the optical absorption of the alkali metal; the photoelectric emission is proportional to this absorption. This picture requires for its test a knowledge of the



SLIDE 12

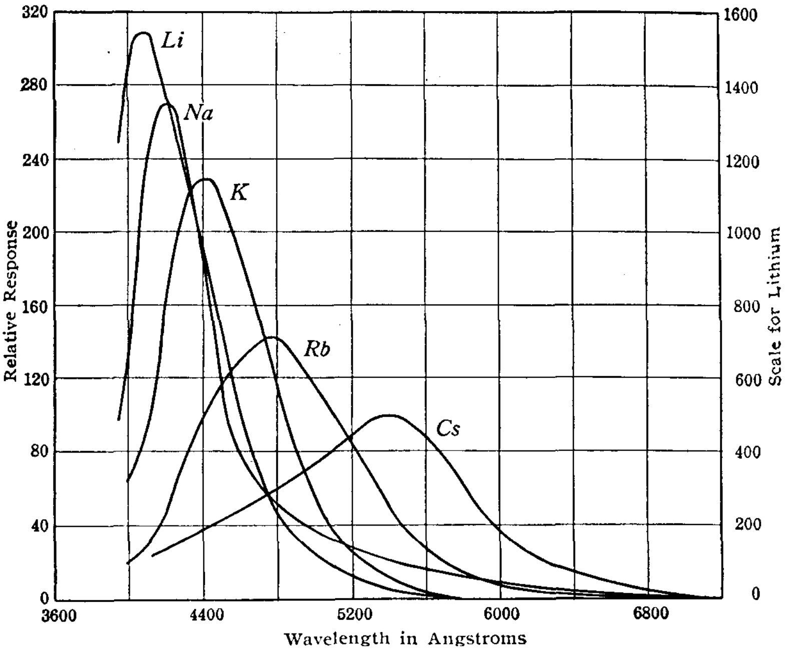
optical constants—the refractive index N, and the extinction coefficient K—of the alkali metals. The determination of these constants presented an experimental problem of considerable difficulty, for the metals must be measured in vacuo and must be prepared so as to have specular surfaces. This investigation of the optical properties of these metals, which is a story in itself, gave a set of data, represented by Slide 15, in which you will see a set of similar N and K curves, moving toward the red, as we go from sodium to cesium. With these curves available, straight application of the theory gave a set of curves of computed photoelectric emission, when calculations were made for a plausible thickness of the sensitive layer, which are shown



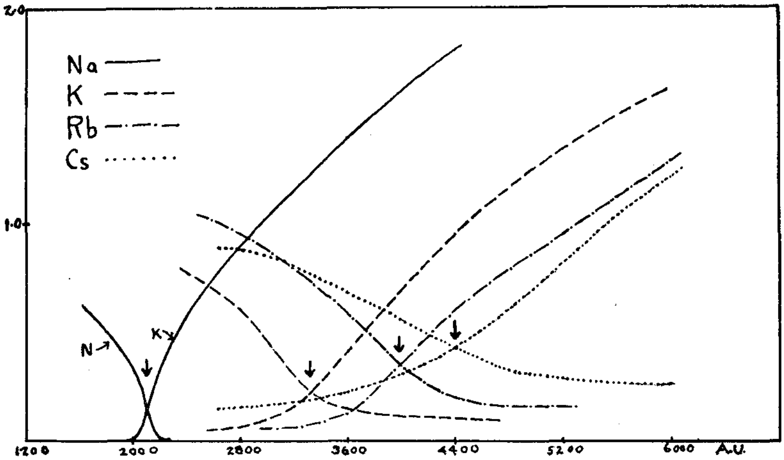
SLIDE 13

in Slide 16. You will note that the progression of maxima toward the red is clearly predicted. The maxima in fact lie at the crossing points of the N and K curves.

When these predictions are compared with experiment, as shown in Slide 17, it appears the true distributions of emission (shown by the solid lines) are somewhat shifted toward the violet, and are less pronounced. This discrepancy is however entirely explainable if we assume, not one and the same thickness

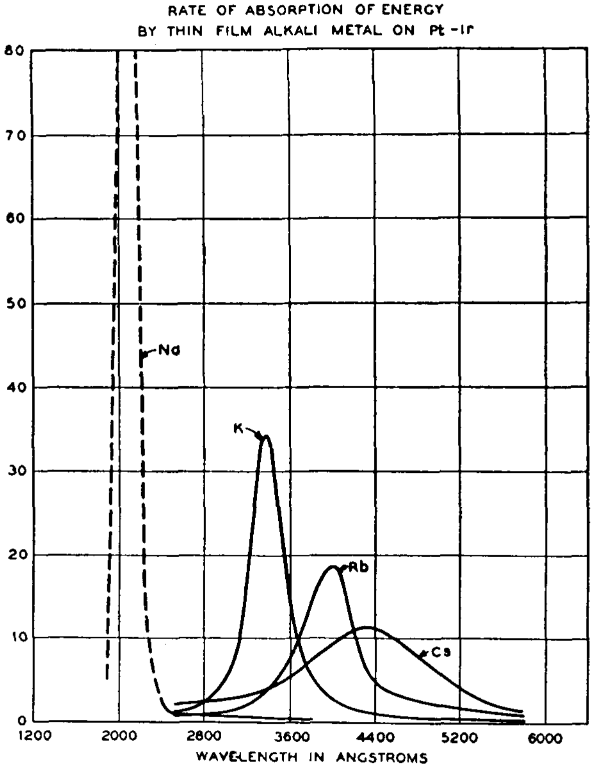


SLIDE 14



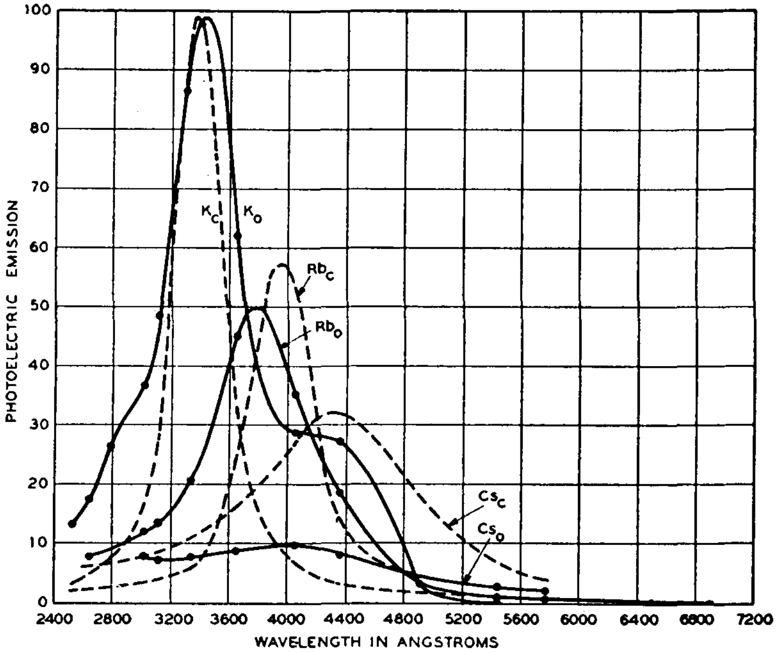
SLIDE 15

for the thin spontaneously evaporated films, but different thicknesses for the different metals. How closely the fit can be made between calculated and experimental emissions is shown in the next two slides. Slide 18 shows (dashed line) the computed and (full line) the observed emission for rubidium; Slide 19 the same data for cesium.

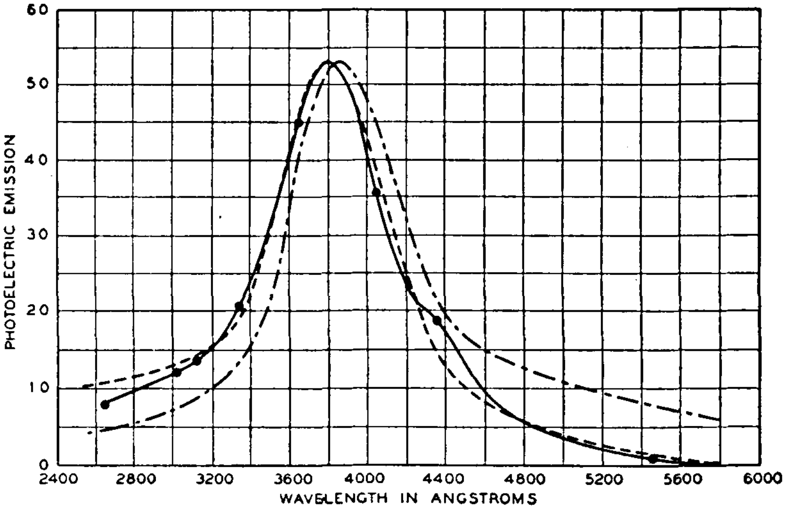


SLIDE 16

The significance of these experiments, which I wish to impress upon you, is that it is the *optical* properties of the materials, and the optical conditions of the region in which the phenomena occur, which explain these outstanding effects. I stress this because other attempts at explanation, which have



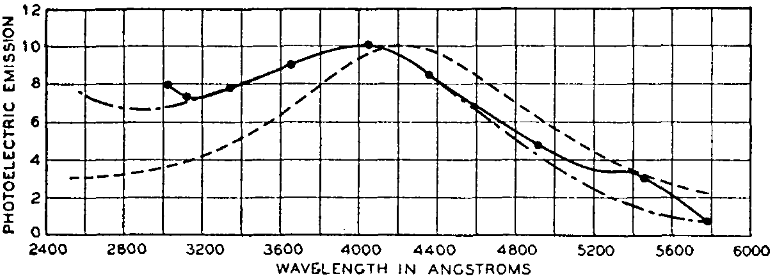
SLIDE 17



SLIDE 18

been made from quantum mechanical considerations, have almost uniformly neglected or ignored the optical factors. These, instead of being secondary or negligible, really dominate.

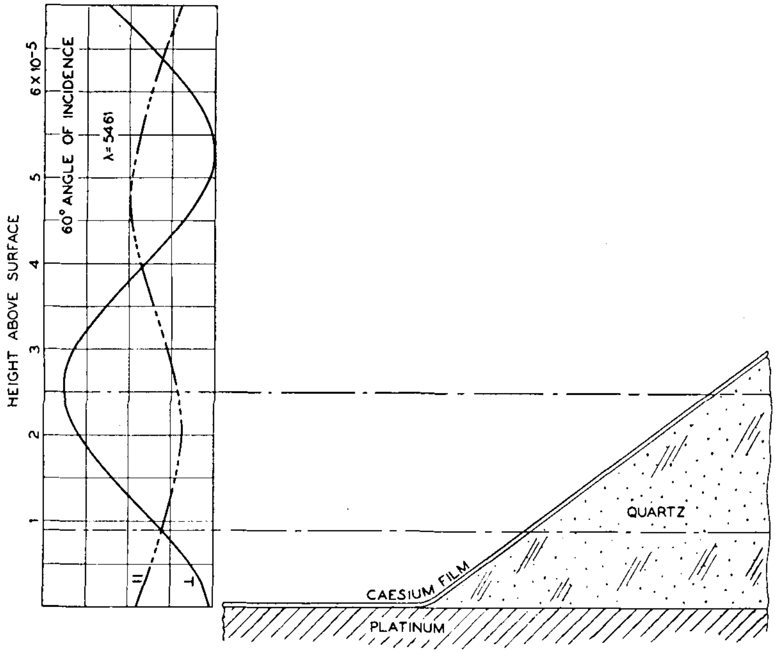
The last experimental adventure with standing waves of light which I want to discuss brings us full circle. It is a repetition of the experiment of Wiener, with which I introduced the subject, but with this difference, that instead of the thin photographic film which Wiener used, a thin photoelectric film was employed. The scheme of the experiment is shown in Slide 20. A quartz wedge was built up on platinum, by evaporation from a quartz-coated tungsten wire. On the platinum and quartz a thin film of cesium was allowed to form. You will see that by this means the standing wave system which thus far we have examined photoelectrically only close to the reflecting surface can be similarly examined for a series of positions above that surface.



SLIDE 19

At the left of the slide is a set of computed curves showing the intensities for the two planes of polarization, for light incident at 60 degrees, at different distances above the platinum. It is evident that we may expect a variety of relations between the emissions at different positions along the wedge and for different polarizations of the incident light. What is here given for one angle of illumination is repeated with variations for other angles, leading to complicated relations, all however susceptible to exact computation.

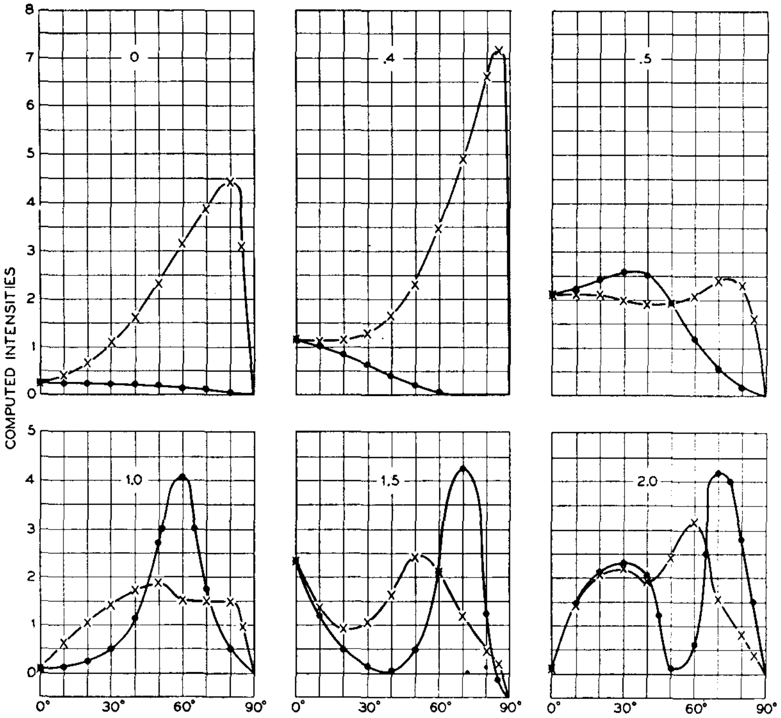
In more detail, we see in Slide 21 what we are led to expect, by computation, for a number of different thicknesses of the wedge, through the complete range of angles of incidence. These relations are most involved, and behaviors are predicted



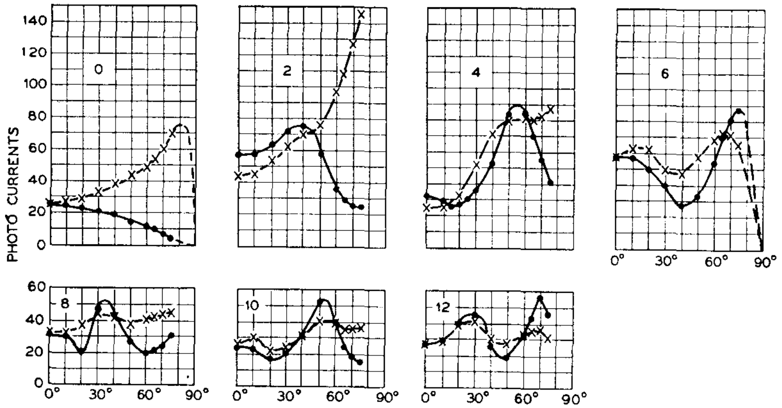
SLIDE 20

never seen before by land or sea. Instead of the simple falling off of emission with angle, or the simple rise to and decline from a maximum, we have actually multiple maxima, with the emissions for the two planes of polarization reversing their positions. On exploring the quartz wedge with a small spot of light, and measuring the photoelectric emissions, we do in fact find all of these predictions borne out, as is shown by Slide 22. The verification of the electromagnetic wave theory of light furnished by this experiment considerably transcends, I think, the original simple experiment of Wiener, conclusive as that was quite properly considered.

I shall devote the remainder of this address, not to experimental adventures with standing waves, but to adventures in the contemplation of these phenomena — to speculations on their significance in the realm of theory. What do they contribute to the old question of the nature of light, to the recurrent question of waves versus corpuscles?



SLIDE 21



SLIDE 22

The last experiment showed how minutely the photoelectric emission followed the predictions made from the wave nature of light. Yet it is from this very photoelectric effect that the wave theory has received its most serious challenge. Philip Lenard, over fifty years ago, in studying the photoelectric effect, found that the energies of the ejected electrons were quite independent of the intensity of the exciting light. Einstein, in 1905, proposed that the energies associated with the photoelectrons be identified with the quantum of energy discovered by Planck, proportional to the frequency of the light. This identification has been abundantly confirmed, notably by the experiments of Millikan. In addition to the idea that a photoelectron carries the energy $h\nu$ Einstein also proposed, as a "heuristic" view, that this amount of energy was also discrete in the radiation before it encountered the electron. This is equivalent to the assumption that light travels in bundles — now called "photons". This is a return to the corpuscular theory toward which Newton is commonly represented as having inclined. It is this photon conception of light which has of late attracted most attention.

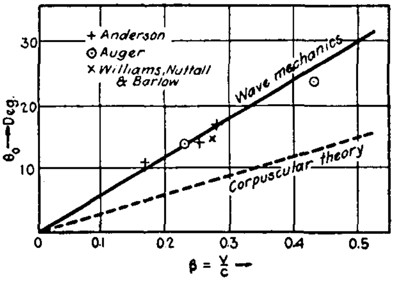
But however attractive from some standpoints the photon appears, it has not been acceptable to those of wide acquaintance with the intricate phenomena of optics. The case was well stated by H. A. Lorentz in an address in 1910, from which I quote the following extract: "Nevertheless the speaker holds the hypothesis of light quanta to be impossible, if the quanta are regarded as wholly incoherent, an assumption which is most natural and which is also made by Planck. The impossibility of incoherent quanta follows from a consideration of interference phenomena. Specifically Lummer and Gehrke have observed interference at a phase difference of two million wavelengths; for yellow light, that corresponds to a length of one meter. If each quantum by itself should be capable of giving sharp interference, then it must also itself extend over that length in the direction of propagation. But, the lateral area of the quantum must also be considerable, which follows from the diffraction theory of optical instruments. Should a light quantum cover only one square centimeter of area, then it would be obviously senseless to fabricate large telescope objectives, for only a small fraction of the area would be used by each quantum in the production of images; on the other hand it is well known that the clearness of images can be greatly enhanced by large objectives. Thus a light quantum would have to be as large as the largest telescope objectives, and, since it is unlikely that the volume of the quantum adjusts itself to our instruments, it would be necessary to conceive of it as considerably larger. But then, only fractions of quanta could get through a small opening such as the pupil of the eye. According to the hypothesis of absorption by the retina, only whole quanta can be taken up, so that fractions would have to recombine into whole quanta. Moreover, the consideration of the simplest interference phenomena, such as Newton's rings, shows that the quanta must be divisible, because in the process the ray is resolved by reflection into two parts which travel by different paths and finally come to interference.''

To this expression of dissent is to be added that of Henri Poincaré, who in his only published reference to Einstein remarks that while Einstein would put the quantum in the incident light, he, Poincaré, would place it in the molecule; and he speaks elsewhere of finding the ''vestibule'' by which the molecule admits or releases energy in quanta.

Now the objections raised by Lorentz, that the phenomena of interference are incompatible with the idea of photons, are encountered in their extreme form, one might say ''head on'', in the phenomena of standing waves. If light consists of streams of photons, then in their reflection at a mirror, if they are incoherent, they must alternately exist and not exist, in the loops and nodes. This is a much harder thing to swallow than the fits of easy and difficult reflection suggested by Newton. If, on the other hand, each photon has a structure adequate to account for interference, it must be a most complex one to account for the existence of polarization and directional effects. This structure would have to be of atomic dimensions to fit the experiments described above. But we must also include in our survey standing waves of all dimensions, among which are mile-long radio standing waves, which would mean that the photons in such radiation must also be miles long. The objections to photons which Lorentz cited in terms of the pupil of the eye and the aperture of astronomical telescopes must, when we consider standing waves, be extended to atomic dimensions at one end and intercity distances at the other. The concept of photons becomes fantastic.

Refuge has been taken from this unsatisfactory state of affairs by using wave descriptions for some phenomena and photon descriptions for others, and it has been claimed that the two types of phenomena are never met in the same experiment. I submit that the last experiment described, the repetition of Wiener's experiment, certainly comes very close to showing both types of phenomena in conjunction. Electrons are emitted with energy $h\nu$, but they follow minutely the orders of the standing wave pattern.

Now the way out of this unsatisfactory situation is I believe furnished by one of the most important developments of physical theory in our time; I refer to the wave mechanics of de Broglie and Schrödinger. This wave mechanics may be looked upon as the happy hunting ground of the student of standing waves. An element of matter according to it *is* a standing wave system. It is not however on the standing wave aspects of wave mechanics that I wish to dwell. It is rather to this fact: that wave mechanics puts the h of quantum theory in the *atom* and not in the *radiation*. It provides in short that "vestibule" that Poincaré demanded, in order to avoid the unacceptable idea of photons.



SLIDE 23

Wave mechanics has rather triumphantly done this when used to develop the theory of photoelectric action, in the hands of Beck and Sommerfeld. Carrying the theory into the region of X rays, they have shown that the distribution in direction of ejected photoelectrons differs significantly when light is considered as waves or as particles. This is illustrated by Slide 23, which shows how the "bi-partition angle" differs by a factor of two for the two cases, and that the experimental results are decisive in favor of the wave mechanical picture as against the corpuscular.

In fairness to Sommerfeld I should say that he expressed his preference for the corpuscular interpretation as furnishing a more ''causal'' explanation than does the wave mechanical. Now why with the facts before him he should cling to a concept which they refute is a problem for the psychologist rather than the physicist. With the background of a student of the interference of light, particularly of standing waves, the more appropriate conclusion would appear to be the sentence with which Lorentz concluded the address to which I have referred above, ''von Lichtquanten, keine Rede sein kann''—''photons just won't do''.

Count Rumford, in his epoch-making communication to the Royal Society of 1798, on the heat produced by friction, posed the question: ''What is Heat? Is there any such thing as an *igneous fluid?* Is there any thing that can with propriety be called *caloric?*'', and concluded, ''it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of any thing, capable of being excited and communicated, in the manner Heat was excited and communicated in these experiments, except it be MOTION''.

It appears to me that we are faced in optics with the same kind of question which Rumford faced in heat. Viewing the phenomena presented by standing waves, it is, to quote Rumford's words, ''extremely difficult, if not impossible'' to retain the idea of light as consisting of discrete photons. I do not minimize the difficulty of arriving at an explanation of all optical phenomena, such for instance as the Compton effect, exclusively in terms of wave transmission and quantum ''vestibules''; nevertheless, on the basis of a long preoccupation with standing waves, I venture to predict that this will be done, and that the photon will go the way of the ''caloric'' that Rumford demolished.

Proceedings of the American Academy of Arts and Sciences

Vol. 81, No. 2, P. 33-100, January, 1952

BIOGRAPHICAL DIGESTS IV: CENTENNIALS AND POLYCENTENNIALS DURING 1952 WITH INTEREST FOR CHEMISTS AND PHYSICISTS

Ernest H. Huntress
Massachusetts Institute of Technology

BIOGRAPHICAL DIGESTS IV: CENTENNIALS AND POLYCENTENNIALS DURING 1952 WITH INTEREST FOR CHEMISTS AND PHYSICISTS

Ernest H. Huntress

Massachusetts Institute of Technology
Cambridge, Massachusetts

The present paper represents the fourth in a series of biographical digests of distinguished chemists, physicists, and other scientists conceived some years ago[1] and more specifically implemented in the present form during this and the three preceding years.[2] This unique approach has proved so useful and has attracted such wide attention that its extension to fields other than science is recommended. Attention is directed to the citations following most of the individual digests. These are restricted to material in the periodical literature and are arranged in a sequence of diminishing merit as arbitrarily judged by this writer. When book biographies of a particular individual exist these are often mentioned in the text of the digests.

The same simple notational methods previously explained are continued and the chronological occurrence of the several anniversaries is tabulated, but in order to facilitate easy reference to individual subjects the corresponding sketches have been placed in alphabetical sequence. In view of the substantial collection of these digests now available a cumulative name index including all subjects treated in this paper together with its three precursors has been supplied.

In view of inevitable limitations of time and space, the subjects of these biographical digests have been selected from a host. Among those for whom detailed treatment cannot be presented here the following may be mentioned: Luigi Balbiano (1852-1917), the Italian organic chemist; Maximilian Eugen

---

[1] Huntress, J. Chem. Education *14*, 328-344 (1937).

[2] a. Huntress, Biographical Digests: I (for 1949), Proc. Am. Acad. Arts Sci. *77*, 33-54 (1949); b. II (for 1950), *ibid.* *78*, 1-35 (1950); c. III (for 1951), *ibid.* *79*, 3-44 (1951).

Hermann Dennstedt (1852-1931), German analytical and forensic chemist; Robert Gnehm (1852-1926), dyestuff chemist of Switzerland; Alfred John Greenaway (1852-1938), British chemist and former editor of the Journal of the Chemical Society of London; John Griscom (1774-1852), early American chemist; Josef Emmanuel Hibsch (1852-1940), German mineralogist and geologist; Wilhelm Hisinger (1766-1852), Swedish geologist and mineralogist; Gerardus Johannes Mulder (1802-1880), Dutch biochemist; Franz Oppenheim (1852-1929), German industrial chemist; Carl von Rechenberg (1852-1926), German distillation expert; Eberhard Rimbach (1852-1933), German chemist; Eduard Vongerichten (1852-1930), German chemist; and Leonardo da Vinci (1452-1519), Italian artist, engineer, inventor, and scientist. Although the last lies somewhat beyond the scope of the present series, attention is called to the fact that 1952 represents the 500th anniversary of da Vinci's birth.

Of the twenty-eight subjects comprising this treatment for 1952, twenty-one represent 100th anniversaries, five 150th memorials, with one 200th and one 350th commemoration. From the geographical viewpoint they are distributed among the following countries: Great Britain 7, France 7, Germany 6, United States 4, with one each from Finland, Holland, Russia, and Spain.

MEMORIAL DATES DURING 1951

Jan.	1	Centenary of birth of E. A. Demarcay
Jan.	5	Centenary of birth of A. L. Étard
Jan.	9	Centenary of birth of W. H. Nichols
Feb.	2	Sesquicentenary of birth of J. B. Boussingault
Feb.	6	Sesquicentenary of birth of (Sir) Charles Wheatstone
Mar.	10	Centenary of birth of R. Anschütz
Apr.	14	Centenary of birth of J. M. Stillman
May	1	Centenary of birth of S. Ramon y Cajal
May	2	Sesquicentenary of birth of H. G. Magnus
May	2	Centenary of birth of F. A. Gooch
July	2	Centenary of death of Thomas Thomson
Aug.	1	Centenary of birth of E. E. G. Wiedemann
Aug.	4	Centenary of birth of (Sir) J. J. Dobbie
Aug.	7	Sesquicentenary of birth of G. H. Hess
Aug.	11	Centenary of birth of H. B. Dixon
Aug.	15	Centenary of death of J. Gadolin
Aug.	30	Centenary of birth of J. H. van't Hoff

Sept. 9	Centenary of birth of J. H. Poynting
Sept. 18	Bicentenary of birth of A. M. Legendre
Sept. 28	Centenary of birth of Henri Moissan
Sept. 30	Sesquicentenary of birth of A. J. Balard
Oct. 2	Centenary of birth of (Sir) William Ramsay
Oct. 9	Centenary of birth of Emil Fischer
Nov. 19	Centenary of birth of C. T. Kingzett
Nov. 20	Sesquitercentenary of birth of O. von Guericke
Nov. 28	Centenary of birth of O. P. Fischer
Dec. 15	Centenary of birth of A. H. Becquerel
Dec. 19	Centenary of birth of A. A. Michelson

ANSCHÜTZ, RICHARD

Born March 10, 1852, at Darmstadt, Germany; died Jan. 8, 1937, at Darmstadt, Germany, age 85.

This German organic chemist, universally recognized as one of the most distinguished of the later 19th and earlier 20th centuries, represents another instance of long and active devotion to research and teaching primarily at a single university. His formal professional training began (1870) at the Darmstadt Polytechnic, continued (1872-74) at the University of Heidelberg under Robert Wilhelm Bunsen (1811-1899), Gustav Robert Kirchhoff (1824-1887), and Hermann Kopp (1817-1892), and culminated (1874) in the usual doctorate. Following a subsequent (1874-75) year at the University of Tübingen under Rudolf Fittig (1835-1910), Anschütz associated himself with the University of Bonn where in various capacities he served (1875-1922) for 53 years. He first served as assistant to Friedrich Auguste Kekulé (1829-1896), with whom he was closely associated for more than two decades and whose biographer he ultimately (1929) became. Upon the departure (1882) of Ludwig Claisen (1851-1930) for England, Anschütz took over the instructional post thus relinquished and began independently to receive doctoral candidates eventually numbering more than 100. He soon (1889) received the title of extraordinary professor and upon the death (1896) of Kekulé became acting director of the Bonn Chemical Institute. Two years later (1898) Anschütz officially succeeded both to Kekulé's chair and to the directorship of the Institute. During his subsequent 24-year tenure of these posts until his retirement (1922), Anschütz not only maintained and extended the research productivity of the group but planned and supervised the construction of a magnificent new laboratory. [For a substantial account of this enterprise see Chem. Ztg. *26,* 1025-1029 (1902); more details together with a large photograph of the building may be found in a 71-page quarto volume (1904) by R. Anschütz and R. Schulze (the architect) entitled "Das Chemische Institut der Universitat Bonn"].

No detailed account can be undertaken here of the prodigious experimental contributions of Anschütz to the progress of organic chemistry. Present-day students of this field will be cognizant of the frequency with which his name occurs in their literature searches. Most of the 219 titles comprising his bibliography were published in the *Berichte* or in Liebig's *Annalen*. In addition to the inevitable miscellaneous topics, however, his work may be recognized as mainly concerned with studies on polybasic and hydroxy acids (particularly the maleic-fumaric acid system and its homologs), oxalic acid and its derivatives, higher aromatic hydrocarbons, the behavior of phosphorus chlorides on phenols, phenolic acids, and phenolsulfonic acids, the investigation of salicylides and sulfonylides, the structure of the dyestuff Tartrazine, and the chemistry of "tetronic acid" (3-keto-γ-butyrolactone) [Beil. XVII-403] and "benzotetronic acid" (4-hydroxycoumarin) [Beil. XVII-488].

Anschütz was first [Ber. *25,* 3506-3513 (1892); Ann. *273,* 73-96 (1893)] to obtain in tetrasalicylide and polysalicylide [Beil. X-62] polymers of the non-existent salicylic acid lactone. The tetramer crystallized from chloroform as an addition product of composition $(C_7H_4O_2)_4 \cdot 2\ CHCl_3$ which upon heating gave off its solvent of crystallization furnishing chloroform characterized by exceptional purity and stability and therefore well suited to pharmaceutical and medical employment. Anschütz very early recognized the special value to organic chemists of vacuum distillation and published (1887) a 32 page brochure entitled "Destillation unter vermindertem Druck im Laboratorium" which greatly hastened the general adoption of a practical method now regarded both as routine and indispensable. His name is also still associated with those short-range instruments having glass enclosed scales and universally designated as "Anschütz thermometers".

In addition to the activities associated with the direction of original experimental research, Anschütz also carried out two different types of literary enterprise. Over a period of forty years he collaborated in numerous revisions of the textbook of organic chemistry originated by Victor von Richter (1841-1891). The first volume of Anschütz's revision appeared (1894) as Richter's

7th edition; the second volume of its 12th edition was published (1935) when Anschütz was 83 years old.

Anschütz was a great lover of nature, made many journeys to the Alps, and was particularly interested in vulcanology. At the time (1906) of the great eruption of Mount Vesuvius he made a quick and uninterrupted trip to Naples to observe this unusual phenomenon at first hand. He married a daughter of the famed Bonn physiologist Edward Pflüger (1829-1910) and their elder son Ludwig Anschütz (1899-) became (1930) professor of organic chemistry at the German Technische Hochschule of Brunn. Richard Anschütz himself was one of the oldest members of the German Chemical Society and served (1918-20) as one of its vice presidents. In later life he was honored (1935) by election as a foreign honorary member of the Royal Society of Edinburgh.

P_1B Ber. *74-A,* 29-74 (1941) (incl. bibliog.): P_3B Z. angew. Chem. *45,* 201-203 (1932): B Ber. *65-A,* 78-79 (1932); *70-A,* 65-66 (1937): B Proc. Roy. Soc. Edinburgh *57,* 400-401 (1936-37).

BALARD, ANTOINE JEROME

Born Sept. 30, 1802, at Montpellier in the Department Herault, Southern France; died April 3 (?), 1876, at Paris, France, age 74.

This French inorganic chemist and pharmacist is best known as the discoverer of bromine. After earlier education at the College of Montpellier, he became (1819) at age 17 *préparateur* at the École de Pharmacie whose diploma he subsequently (1826) obtained. After an appointment (1833) as professor of physics in the Montpellier School of Pharmacy and professor of chemistry in the Faculty of Science, he eventually (1842) succeeded L. J. Thenard (1777-1857) at the Sorbonne. He was elected (1844) to the French Academy of Sciences and was appointed (1846) superintendent of lectures at the École Normale Supérieure, a post which later led to his appointment as inspector-general of higher education. The last two decades of his professional career were spent at the Collège de France in Paris where (1851-76) he succeeded T. J. Pelouze (1807-1867) in the chair of general chemistry.

Balard's discovery of bromine was made at the age of 24 while he was still an obscure assistant at Montpellier. Interested in a study of saline brines from the local marshes, Balard found that after suitable treatment with chlorine a heavy dark red oil could be separated by distillation or by other extraction. Eventually, after extensive studies of its properties, convinced of the elementary character of his new material, Balard named it bromine (from the Greek for "stench") and published [Ann. chim. phys. (2) *32,* 337-384 (1826); cf. Chem. News *99,* 205-206 (1909)] a full account of his discoveries. The importance of his work was promptly recognized by the Royal Society of London in the award (1830) to him of a Royal Medal although it never elected him to actual membership.

By one of those not infrequent coincidences of science, bromine was also almost simultaneously but quite independently discovered by Karl Jacob Löwig (1803-1890). Upon entering the University of Heidelberg to study under Leopold Gmelin (1778-1853), the 22-year-old Löwig brought from his home at Kreuznach, Germany, samples of a dark red liquid which he had obtained from certain natural brines by chlorination. Before further investigation could be completed, however, the publication of Balard's results gave priority of discovery to the French chemist.

In addition to the discovery of bromine, Balard discovered hypochlorous acid, worked out the constitution of Javelle water, was the first to regard bleaching powder as a double compound of calcium chloride and hypochlorite. He devised (1840) a method for the extraction of potassium salts from sea water but the economic significance of this work was soon afterward neutralized by the discovery of the great Stassfurt deposits.

P_2B J. Chem. Education *3,* 382-384 (1926); *9,* 1925-1928 (1932); *15,* 255 (1938): B J. Chem. Soc. *31,* 512-514 (1877): B Nature *13,* 475 (1876): B J. pharm. chim. (8) *4,* 563-565 (1926): B J. Am. Pharm. Assoc. *23,* 1024-1026 (1934).

BECQUEREL, (ANTOINE) HENRI

Born Dec. 15, 1852, in Paris, France; died Aug. 25, 1908, at Le Croissic, France, age 56.

This French physicist represented the third generation of a remarkable family from which four members have made distinguished contributions to science. The earliest of this quartet, Antoine César Becquerel (1788-1878), was a pioneer of electrochemistry. The second, (Alexandre) Edmond Becquerel (1820-1891), is known chiefly for his researches on phosphorescence.

The third, (Antoine) Henri Becquerel (1852-1908), is the principal subject of the present digest. The group is currently completed by the latter's son, Jean Antoine Edward Marie Becquerel (1878-). All were physicists, all made important discoveries and inventions, all were for long periods connected with the Paris Muséum d'Histoire Naturelle.

Henri Becquerel received his early education at the Lycée Louis le Grand. At the age of 20 he entered (1872) l'École Polytechnique but two years later (1874) continued his studies at l'École des Ponts et Chaussees, of which afterward he was for a decade the chief engineer. Subsequently, he was appointed (1878) assistant at the Paris Museum of Natural History under his father, Edmond Becquerel, and eventually succeeded (1892) to its Professorship of Physics, a chair already rendered illustrious by his father and grandfather, and earlier by Gay-Lussac (1778-1850) [for a brief account of this laboratory at this time see Nature *71*, 177-178 (1904)]. He also succeeded (1895) his father in the chair of physics at l'École Polytechnique.

Although with his father (Edmond), Henri Becquerel published (1879-83) a series of memoirs on the temperature of the earth, most of the scientific contributions of his earlier period were concerned primarily with light. He was first to observe rotatory magnetic polarization in gases and discovered the magnetic rotation of the plane of polarized light by the earth's magnetic field. He conducted experiments (1886-90) on the absorption of light by crystals, including the anomalies along different axes, and utilized this work in what amounted to a new method of spectrum analysis. He extended his father's studies of phosphorescence, and discovered the laws relating the exciting radiation to the emitted phosphorescence as well as that expressing the diminution of the latter with time. The work for which he is most famous, however, was his discovery (1896) that uranium and its compounds emit invisible but penetrating radiations which can affect photographic plates and produce electrical conductivity in gases. His earliest observations, announced at a meeting of the French Academy of Sciences on Feb. 24, 1896 [Compt. rend. *122*, 420-421 (1896)], represent the first recognition of what was later named radioactivity by Madam Curie. Indeed, it was at the suggestion of Becquerel that Marie Curie undertook for her immortal doctoral thesis (1903) at the University of Paris the study of the radiations produced by a large number of minerals and was thus, with her husband, led through the results obtained from pitchblende to the discovery of polonium and radium. Although the radiations whose effects were first discovered by Becquerel are collectively designated in his honor as Becquerel rays, they are now known from the work of Sir Ernest (Lord) Rutherford (1871-1937) and others to comprise three different kinds, viz. α-rays consisting of helium atoms each bearing two positive charges; β-rays consisting of streams of negatively charged electrons; and γ-rays, identical with x-rays. [For an important résumé of the history of the discovery of Becquerel rays see Naturwissenschaften *36,* 129-132 (1949); for Becquerel's own account (in French) see Proc. Roy. Institution Gt. Brit. *17,* 85-94 (1902-04].

Henri Becquerel's genius was recognized by many honors. He was elected (1889) to the French Academy of Sciences at the age of 37, and later (1908) became its President. A few months later he was named its Perpetual Secretary but served only six weeks as such. It is curious that three scientists honored by election to this post all died in the year of their appointments, viz. the chemist P. E. M. Berthelot (1827-1907), the geologist A. A. C. deLapparent (1838-1908), and the physicist Henri Becquerel (1852-1908). He served (1897) as president of the Société Français de Physique and was elected an honorary foreign member of the Royal Society of London (1908), a foreign associate of the U. S. National Academy of Sciences, and the Royal Institution of Great Britain (1899). Other honors include that of the Rumford Medal (1900) of the Royal Society, the Barnard Medal (1905) of the U. S. National Academy of Sciences, the Helmholtz Medal (1901), and the joint award (1903) together with Pierre Curie (1859-1906) and Marie Curie (1867-1934) of the Nobel Prize in Physics.

P_1B J. Chem. Soc. *101,* 2005-2042 (1912) (Memorial Lecture) [also reprinted in Vol. II, pp. 217-254 (1914) of Memorial Lectures delivered before the Chemical Society (1901-1913), published by Gurney and Jackson, London, 1914]: P_1B Proc. Roy. Soc. (London) *83-A,* xx-xxiii (1909): B Nature *78,* 414-416 (1908): P_1B Smithsonian Institution, Ann. Rept. for 1908, 769-785 (1909) [translated from Rev. gen. sci. pur. appl. *19,* 802-813 (1908)]: B Naturwissenschaften *36,* 129-132 (1949): B Compt. rend. *147,* 443-451 (1908): B Bull. muséum natl. his. nat. (Paris) *11,* 206-226 (1939); *21,* 648-662 (1949): B Ber. *41,* 3277-3288 (1908): B Jahrb. Radioakt. Elektroniks *5,* 391-394 (1908).

BOUSSINGAULT, JEAN BAPTISTE (Joseph Dieudonné)

Born Feb. 2, 1802, in Paris, France; died May 11, 1887, in Paris, age 85.

Of this French scientist, famous not merely as chemist, physicist, mining engineer, but particularly as agronomist, deplorably little information is available. His scientific education was obtained at the School of Mines of St. Étienne, from which at the age of 19 he published [Ann. chim. phys. (2) *16,* 5-16 (1821)] a memoir on the chemistry of silicides of platinum. Shortly thereafter he was appointed by the Spanish government to a professorship at the mining school of Bogotá, Colombia, South America, in which country he remained for a decade. Upon the outbreak of the revolt which ultimately deprived Spain of all her South American domains, Boussingault joined the rebels and became attached to the staff of the famous Símon Bolívar (1783-1830). After the cessation of hostilities, Boussingault engaged in technical mining work and traveled extensively in the region. It was during this period that having noted [Ann. chim. phys. (2) *30,* 91-96 (1825); *48,* 41-69 (1831)] the prevalence of goiter in some native communities as contrasted with its rareness in others, he analyzed the salt used by the two groups, concluded that lack of iodine was responsible for goiter, and thus was led (1833) to recommend [Ann. chim. phys. (2) *54,* 163-178 (1833)] the use of "iodized" salt. At this time also he made the acquaintance of Alexander von Humboldt (1769-1859), who warmly praised Boussingault's chemical, meteorological, geographical, and astronomical accomplishments.

Upon his return to France, Boussingault served briefly as professor of chemistry at the University of Lyons, but soon accepted the chair of agriculture at the Conservatoire des Arts et Métiers (School of Arts and Trades) in Paris, a position which he held for 48 years (1839-87) until his death. In the same year (1839), he was elected to the Institute of France, i.e., to the French Academy of Sciences, of which save for M. E. Chevreul (1786-1889) he was doyen. He served for three years (1848-51) as a member of the French National Assembly, but as a result of his political opinions was dismissed (1851) from his post at the Conservatoire; this summary action was, however, so vigorously protested by Boussingault's professional colleagues as to compel his reinstatement by the Government. Although he never became a member of the Royal Society of London, he was honored (1878) by its highest distinction, the Copley Medal, bestowed in recognition of his varied contributions to science, particularly those connected with the development of agriculture. A monument to his memory was erected (1895) in the courtyard of the Conservatoire [for photograph see J. Chem. Education *11,* 216 (1934)]. A portrait of Boussingault is included in H. M. Smith's "Torchbearers of Chemistry", Academic Press, N. Y., 1949, page 36.

Soon after Boussingault's return from South America he married the sister of a former fellow student at St. Étienne and became joint proprietor with his brother-in-law, the father of J. A. LeBel (1847-1930) of subsequent stereochemical fame, of the estate of Pechelbronn in Alsace-Lorraine. Here he established a laboratory, which carried out a long series of important agricultural chemical researches, and has been recognized as constituting the first agricultural experiment station in the modern sense. Among the topics developed by Boussingault were studies of the principles underlying the merits of proper rotation of crops, investigations of the nutritional values of various diets for herbivorous animals, determination of utilization of various constituents of animal fodders, etc. Boussingault showed that plants do not absorb nitrogen from the air but that their content of this element may be completely accounted for in the corresponding fertilizers. As early as 1843 Boussingault published his "Économie Rurale" which, translated and republished (1845) in England under the title "Rural Economy in its Relation with Chemistry, Physics, and Meteorology," was characterized as the most valuable contribution of the century to agricultural chemistry. Much later (1860-74) he assembled most of the results of his researches in the monumental seven-volume "Agronomie, Chimie agricole et Physiologie", of which a second edition (1884) was later required.

B J. Chem. Soc. *53,* 509-513 (1888): B Nature *19,* 39-40 (1879); 36, 134-135 (1887): B Chem. News *55,* 243 (1887): B Pop. Sci. Monthly *33,* 836-841 (1888): B Compt. rend. *104,* 1339-1345 (1887).

DEMARCAY, EUGENE ANATOLE

Born Jan. 1, 1852, at Paris, France; died 1904, at Paris, France, age 52.

Although this French chemist was the discoverer of one of our rarest elements, little record has been made of his career. It is known that after early education at the Lycée Condorcet and a year in England he entered (1870) the École Polytechnique.

Here he became the pupil of A. A. T. Cahours (1813-1891) to whom he was also for six years (1874-80) an assistant, meanwhile also gaining the friendship of other French savants including C. A. Wurtz (1817-1844), H. St. Clair-Deville (1818-1881), J. B. A. Dumas (1800-1884), Charles Friedel (1832-1899), A. M. Cornu (1841-1902), Paul Schutzenberger (1829-1897), and Lecoq de Boisbaudran (1838-1912). Upon leaving the École Polytechnique he made an extensive trip through Algeria, Egypt, and India, returned to Paris, and devoted the remaining two decades of his life to research in pure sciences.

His earlier papers were almost exclusively concerned with organic chemistry. His study of the essence of Roman camomile, one of the earliest attempts to recognize the components of complex natural oils, led to his isolation of hemiterpenes and esters of unsaturated acids eventually of value to the perfume industry. With Cahours he studied the esterification of primary, secondary and tertiary alcohols with oxalic acid, various organometallic compounds of tin, and independently made pioneer investigations of ethyl acetoacetate and its utility in organic synthesis.

After leaving Cahours' laboratory, however, all of his independent research was confined to inorganic chemistry. At the age of 28 during some experiments on the sulfides of nitrogen, the explosion of an iron pressure vessel cost Demarcay the sight of one eye but despite this handicap he eventually became a noted spectroscopist. It was said that he would read a complex spectrum "like an open book" and was in fact the first to observe the new lines of radium in samples brought by Pierre Curie for his examination. At his laboratory in Paris on the Boulevard Berthier, Demarcay assembled high-vacuum apparatus with which he carried out studies on the volatility of metals such as zinc, cadmium, and gold at very low temperatures and pressures. From about 1885 he devoted himself to studies of the rare earths and became a recognized master of methods for effecting their separation. He exhibited (1900) a remarkable collection of pure crystalline salts of neodymium, praseodymium, and samarium and in the following year (1901) discovered europium.

P_2B Bull. soc. chim. (3) *31,* No. 4, i-viii (1904) (incl. bibliog.): B Chem. News *89,* 137-139 (1904) (English translation of preceding reference but lacks bibliog.): P_3B J. Chem. Education *9,* 1767-1769 (1932).

DIXON, HAROLD BAILY

Born Aug. 11, 1852, at London, England; died Sept. 18, 1930, at Manchester, England, age 78.

This British authority on flame and explosion was the second son of William H. Dixon (1821-1879), traveller and historical writer. After early education (1867-71) at Westminster School, London, he entered Christ Church, Oxford, where his student career (1871-75) was diverted from the classics to science by A. Vernon Harcourt (1834-1919). He was elected to fellowships at Trinity College and at Balliol and taught at both until 1886. He then succeeded Sir Henry Roscoe (1833-1915) in the chair of chemistry at Owens College, Manchester, which for 35 years (1887-1922) he occupied with great distinction. Even after relinquishing to Arthur Lapworth (1872-1941) his teaching responsibilities and becoming professor emeritus, he actively continued research to the end of his life.

Dixon undertook research on gaseous explosions at the instigation of Harcourt, with whom he worked until 1879 in the crypt of one of the monastic buildings on the site of which Christ Church was built and later in the cellar of Balliol which had been used by Benjamin Brodie for his researches on ozone. Of the three periods into which Dixon's researches divide, the first was marked by his discovery (1880) that prolonged drying of a carbon monoxide-oxygen mixture over phosphoric anhydride rendered it nonexplosive to sparks able to ignite an undried mixture. The results of this early Oxford period (1876-81) not only confirmed the validity of Berthollet's "law of mass action" (1805) in gaseous explosions (a conclusion simultaneously and independently established at Heidelberg by Horstman), but also led to the remarkable discoveries regarding the incombustibility of rigorously purified and dried gases by H. B. Baker (1862-1935) who assisted Dixon (1883-85).

In 1880 a disastrous explosion in a 36-inch gas main in Percy and Charlotte Streets and Fitzroy Square near Tottenham Court Road, London, provided dramatic evidence that gaseous explosions travel at higher rates than previously supposed, and Dixon began a systematic investigation of the rate of propagation of gaseous explosions generally. At this juncture M. P. E. Berthelot (1827-1907) and P. Vieille (1854-1934) announced their discovery of the high constant flame speeds attained on detonation. During the next 20 years Dixon so successfully developed the methods initiated by the French that he became the leading authority in this field. In 1893 he gave the Bakerian lecture

[Phil. Trans. Roy. Soc. (London) *A-184*, 97-188 (1894)] on "The Rates of Explosion in Gases" and nine years later published [Phil. Trans. Roy. Soc. (London) *A-200*, 315-352 (1903)] a brilliant memoir embodying his photographic researches on "The Movements of Flame in the Explosion of Gases".

Dixon's third research period (1903-30) was concerned with the ignition temperatures of explosive gaseous media which he was the first to determine with any real accuracy. Late in the period he found that these temperatures are affected by the presence of small amounts of other substances and that iodine vapor markedly raises ignition temperature. It was upon these experiments that Dixon was engaged only a few hours before his death.

Dixon was fond of outdoor sports and played cricket and tennis well into middle life, but his chief physical recreation was mountaineering. In one three-year period (1890-93) he made more than 20 first-class Alpine climbs and later (1897) climbed in the Selkirks, British Columbia, Canada, making first ascents of both Pollux and the Dome and the second ascent of Castor. In the Canadian Rockies he made, with J. Norman Collie and others, the first ascents of Mounts Lefroy and Gordon.

Dixon was elected (1886) to the Royal Society of London and received (1913) one of its Royal Medals. He served as president of the famous Manchester Literary and Philosophical Society (1907-09) and of the (British) Chemical Society (1909-11).

P_1B J. Chem. Soc. *1931*, 3349-3368; also identically in Proc. Roy. Soc. (London) *A-134*, i-xxvi (1932): B Nature *126*, 511-512, 958 (1930): B Chemistry and Industry *8*, 805-806 (1930): B Mem. Proc. Manchester Lit. and Phil. Soc. *75*, ii-iii (1930-31): B J. Chem. Soc. *1930*, Proc. 88-89.

DOBBIE, SIR JAMES JOHNSTON

Born Aug. 4, 1852, at Glasgow, Scotland; died June 19, 1924, at Fairlie, Ayrshire, Scotland, age 72.

This Scottish chemist did most of his work in Wales and England. Entering the University of Glasgow to pursue architecture, he received M.A. (1875) but since his interest then turned to science and Glasgow then offered no science degree, he undertook further study at the University of Edinburgh. Here he came under the influence of Profs. Blackie, Alexander Crum Brown (1838-1922), and (Sir) Archibald Geikie (1835-1924) and attained B.Sc. (1878) and Sc.D. (1879). Meantime, he spent two semesters at Leipzig studying chemistry with A. W. H. Kolbe (1818-1884) and G. H. Wiedemann (1826-1899) and mineralogy with Ferdinand Zirkel (1838-1912). While at Glasgow, Dobbie became acquainted (1875) with William Ramsay (1852-1916), then an assistant to John Ferguson (1837-1916), and later (1881) succeeded him in this capacity. He next occupied for 19 years (1884-1903) the chair of chemistry at the newly founded University College of North Wales at Bangor, Caernarvonshire. With some misgivings he then accepted (1903-09) the post of director of the Royal Scottish Museum at Edinburgh, although during this period he managed to maintain the research program initiated at Bangor. His last professional post was in London (1909-20), where he succeeded Sir T. E. Thorpe as principal of the government laboratory and (after 1911) became the first Government Chemist. He served in this difficult position through World War I, finally retiring in 1920.

Dobbie's contributions to science were primarily in the chemistry of alkaloids and in the development of ultraviolet absorption spectra as a tool for the establishment of organic structures. While both men were at Glasgow (1876), Ramsay and Dobbie jointly initiated an investigation of the structure of quinine, quinidine, cinchonine, and cinchonidine, about which little was then known. By alkaline permanganate they degraded these substances to pyridinetricarboxylic acids and thus first established them as pyridine derivatives. This work comprised Dobbie's doctoral thesis at Edinburgh. At Bangor, Dobbie greatly extended his alkaloid studies, isolated corydaline, corybulbine and corytuberine, and established the structure of corydaline. In collaboration with (Sir) Walter N. Hartley (1846-1913) of Dublin, Dobbie began (1898) to study the relation between structure and ultraviolet absorption spectra of organic compounds and established the great utility of this physical method as a regular procedure. They gave particular attention to the investigation of tautomeric substances such as isatin, carbostyril, cyanuric acid, phloroglucinol and many alkaloids. Some 20 years later Dobbie and Fox extended the work to the absorption spectra of gaseous elements. Other miscellaneous topics studied by Dobbie included the first preparation of *o*-bromochlorobenzene and of diphenylene.

While at Bangor Dobbie recognized the special importance of agricultural education including field experimentation and was instrumental in the founding (1889) of the first agricultural department in connection with a university college. As director of the Edinburgh Museum, he greatly extended its holdings of Egyptian antiquities, the Noel Paton collection of armor, and its sections of natural history and engineering. He served for 14 years as adviser on research programs to the Carnegie Trust for the universities of Scotland, as a member of the Royal Commission on awards to inventors, and in many other public service capacities.

Dobbie served as president both of the Institute of Chemistry (1915-17) and of the Chemical Society of London (1919-21). He was elected to the Royal Societies of Edinburgh and of London in the same year (1904) and knighted (1915) by the Crown.

P_1B J. Chem. Soc. *125,* 2681-2690 (1924) : P_1B Proc. Roy. Soc. (London) *A-107,* vi-viii (1925) : B Nature *114,* 198-199 (1924) : B Chemistry and Industry *2,* 692-693 (1924) : B Proc. Roy. Soc. Edinburgh *44,* 271-272 (1923-24) : B Chem. Age (London) *10,* 670 (1924).

ÉTARD, ALEXANDER LEON

Born Jan. 5, 1852, at Alençon, Orne, France; died May 1, 1910, at Paris, France, age 58.

This French organic chemist spent his boyhood years with his parents in Chile, South America. At the age of 17 he returned to France and, despite a complete lack of any scientific or technical training, persuaded Edmond Frémy (1814-1894) to allow him to work in his laboratory. Here he devoted himself so assiduously to science that upon obtaining his baccalaureate he was recommended by Frémy to Auguste André Thomas Cahours (1813-1891) as préparateur at the École Polytechnique. Here he not only obtained (1880) his doctorate but occupied various posts for some twenty years (1880-1900). Meanwhile, he also held an appointment (1885) as professor at the École Municipale de Physique et de Chimie industrielle. The last decade of his life (1900-10) was spent at the Pasteur Institute of Paris as director of its laboratory of biological chemistry.

Étard's name is chiefly remembered from the Étard reaction. During his doctoral research with Cahours, Étard discovered [Compt. rend. *84,* 127-129 (1877); Bull. soc. chim. (2) *27,* 249-251 (1877)] the action of chromyl chloride (CrO_2Cl_2) on aromatic hydrocarbons and developed it into the well-known synthesis for aldehydes which still bears his name. While associated with Cahours he also carried out research on derivatives of nicotine which laid the basis for its later synthesis by Ame Pictet (1857-1937). Subsequently (1882-84) he collaborated with Armand Emile Justin Gautier (1837-1920) in studying the ptomaines formed during the putrefaction of albuminoids. He also discovered [Compt. rend. *112,* 945-947 (1891)] the hydrocarbon now known as cyclopentadiene and its spontaneous polymerization to dicyclopentadiene.

P_1B Bull. soc. chim. (4) *9,* No.?, i-xix (1911) (incl. bibliog.)

Fischer, (Hermann) Emil

Born Oct. 9, 1852, at Euskirchen (20 miles from Bonn), Rhenish Prussia, Germany; died July 15, 1919 [cf. Z. angew. Chem. *32,* (Aufsatzteil), 360 (1919)] at Wansee, Berlin, Germany, age 67.

This German organic chemist, possibly in scope and magnitude of his accomplishments the greatest in history, received his early education in Bonn and at age 17 spent two years as an apprentice in his brother-in-law's timber business. His desire to pursue a scientific career, however, led him to become (1871) a pupil of Friedrich Auguste Kekulé (1829-1896) at the University of Bonn. In the following year he transferred to the University of Strassbourg and after two years (1872-74) with Adolf von Baeyer (1835-1917) obtained his doctorate (1874) with a dissertation [Ber. *7,* 1211-1216 (1874)] on fluorescein and orcinolphthalein. When at this juncture von Baeyer succeeded to the chair of Justus von Liebig (1803-1873) at Munich, Emil Fischer accompanied him as assistant and during his subsequent seven year (1875-82) stay, presently (1879-82) succeeded Jacob Volhard (1834-1910) as director of the analytical division. At the age of thirty, Fischer again succeeded Volhard at the University of Erlangen but remained here only three years (1882-85). His next academic post was at the University of Würzburg, where he spent seven years (1885-92) in the chair of chemistry as the successor of Johannes Wislicenus (1835-1902). During this Würzburg period was born the eldest of his three sons, Hermann Otto Laurenz Fischer (1888-), currently (1951) in the U. S. A. at the Department of Biochemistry of the University of California at Berkeley. Upon the death of August Wilhelm von Hofmann (1818-1892), the 40-year-old Emil Fischer succeeded him as professor and director of the Chemical Institute of the University of Berlin, where he spent the remaining 27 years (1892-1919) of his life.

Although Fischer's subsequent professional career was primarily concerned with three major fields, viz. sugars, purines, and the interrelation of amino acids, polypeptides and proteins, several earlier accomplishments require brief mention. Although these were executed during the Munich period before he was thirty years old, their consequences were profound both upon organic chemistry and in attracting attention to the man himself. In the year following his doctorate, he first prepared [Ber. *8,* 589-594 (1875); Ann. *190,* 67-183 (1877)] phenylhydrazine, the substance which in his own hands was ultimately to prove the key to the chemistry of the sugars. While in Munich he also collaborated with his cousin (Philip) Otto Fischer (1852-1932) in establishing (1878) the rosaniline dyes as derivatives of triphenylmethane. His doctoral thesis on certain phthalein dyes together with the discovery of phenylhydrazine and the elucidation of the structure of the rosaniline dyestuffs represented Emil Fischer's only concern with the chemistry of organic compounds of the aromatic series.

The beginning of Emil Fischer's colossal contribution to the chemistry of carbohydrates occurred (1884) ten years after his discovery of phenylhydrazine through his recognition [Ber. *17,* 579-584 (1884)] of the class of phenylosazones formed by reaction of simple carbohydrates with an excess of phenylhydrazine. Although the stages of this reaction were at first obscure, he soon [Ber. *20,* 821-834 (1887)] discovered the phenylhydrazones and clarified their role in the reaction. Whereas the initial sugars themselves were so extremely water soluble that the resulting sirups were virtually impossible to characterize, their phenylosazones and phenylhydrazones could readily be recrystallized and purified and generally were much more suited to chemical and physical study than the corresponding parent carbohydrates. Utilizing this entirely new tool and devising many other new methods and techniques, Fischer synthesized not only many of the natural sugars but also produced numerous individual carbohydrates unknown in nature. By application of the stereochemical views of Jacobus Henricus van't Hoff (1852-1911) and of Joseph Achille LeBel (1847-1930) to the problem of the aldohexoses, he was able (1891) to allocate the appropriate configuration to all sixteen possible optical isomers, twelve of which were either synthesized or configurated or both in his laboratory. When it is recalled that in 1886 only two aldohexoses (glucose and galactose) were recognized, and that nothing was known of the configurations of the four asymmetric carbon atoms possessed by each aldohexose, the magnitude of Fischer's contribution is at least suggested. [For an account of Fischer's discovery of the configuration of glucose see C. S. Hudson, J. Chem. Education *18,* 353-357 (1941).] His work with the various smaller units of the carbohydrate group was gradually extended to their combinations with each other and with entirely different molecules. He discovered (1893) the first methylglucoside and subsequently synthesized many natural and artificial glucosides and polysaccharides. This work eventually led him to studies of the depsides and tannins, in the course of which Fischer and Karl Freudenberg (1886-) prepared [Ber. *46,* 1116-1138 (1913)] hepta-(tribenzoyl-galloyl)-p-iodophenylmaltosazone, whose molecular weight (4021) vastly exceeded that of any other synthetic product prepared up to that time.

The second major topic of Emil Fischer's life work was concerned with a group of organic compounds to which he gave (1884) the class designation of "purines". This group includes not only uric acid, caffeine, theobromine, and theophylline but also many other substances of physiological interest. In a long series of researches extending over about two decades (1881-1899), Fischer elucidated the structure and effected (or vastly improved) the synthesis of many members of this family, including the first preparation [Ber. *31,* 2550-2574 (1898)] of the parent purine itself. At various times during his pursuit of this topic he established the structure of adenine as 6-aminopurine, of hypoxanthine as 6-hydroxypurine, of guanine as 2-amino-6-hydroxypurine, of xanthine as 2,6-dihydroxypurine, of theophylline as 1,3-dimethyl-2,6-dihydroxypurine, of theobromine as 3,7-dimethyl-2,6-dihydroxypurine, of caffeine as 1,3,7-trimethyl-2,6-dihydroxypurine, and of uric acid as 2,6,8-trihydroxypurine. Fischer's service did not lie in the discovery of the various purines (for example, uric acid had been discovered (1776) by Carl Wilhelm Scheele (1742-1786) and caffeine first isolated (1820) by Friedlieb Ferdinand Runge (1795-1867)), but rather in his establishment of the important representatives as simple derivatives of one fundamental unit (purine), and in the synthetic preparation of numerous other new members of the same family.

The third great contribution of Emil Fischer to organic chemistry and to biochemistry was his extension and development of knowledge of amino acids, peptides, and proteins. When he began (1899) his work in this area it was known that proteins could by hydrolysis or enzymatic disruption be broken down to amino acids, and the racemic forms of thirteen such fundamental units had thus been obtained. Furthermore, the synthesis of the racemic forms of eight of these had already been effected; viz. alanine (α-aminopropionic acid) (1850), leucine (α-aminoisocaproic acid) (1855), glycine (α-aminoacetic acid) (1858), valine (α-aminoisovaleric acid) (1880), phenylalanine (1882), tyrosine (α-amino-β-(p-hydroxyphenyl)propionic acid (1882), aspartic acid (α-aminosuccinic acid) (1887), and glutamic acid (α-aminoglutaric acid) (1890). The resolution of amino acids into their optically active stereoisomers, however, had been severely limited by experimental difficulties attributable to their amphoteric nature. By repressing their basic character through various means of acylation, however, and thus facilitating their capacity to form salts with natural optically active alkaloids, Fischer and his collaborators were able to obtain for the first time the antipodal components of twelve amino acids including from the preceding group alanine, leucine, valine, phenylalanine, tyrosine, aspartic acid and glutamic acid and beyond it the cases of α-aminobutyric acid, α-amino-n-caproic acid, serine, isoserine, and proline. These materials thus became the building blocks for his later work on their recombination into polypeptides.

Earlier attempts to recognize the individual amino acid components of proteins had also been seriously impeded by experimental obstacles to the separation of the complex mixtures produced by hydrolysis. Although Theodor Curtius (1857-1928) had recognized (1883) that in consequence of the suppression of their carboxylic acid function through esterification the resultant amino acid esters had the properties of aliphatic amines, it was Emil Fischer who modified, developed, and extended this principle into a powerful new tool for their separation from complex mixtures by distillation of their ethyl esters under reduced pressure. The direct result of this increased knowledge of amino acids and their derivatives was the recognition and determination of the components of a wide variety of natural proteins, including special attention (1907) by Fischer himself to the fibroin produced by silkworms and spiders.

Utilizing the vast experience thus acquired through his work on the properties of amino acids and their isolation from protein hydrolysates, Fischer next made an even more important advance in his synthetic studies of the recombination of amino acid units. The recognition of hippuric acid as *N*-benzoylglycine having given a clue to the mode of linking, and the preparation [Ber.

34, 2868-2877 (1901)] of the first dipeptide (glycyl-glycine) supporting this view, Fischer entered upon that remarkable series of polypeptide syntheses which culminated [Ber. *40,* 1754-1767 (1907)] in the preparation of an octadecapeptide, viz. *l*-leucyl-triglycyl-*l*-leucyl-triglycyl-*l*-leucyl-octaglycyl-glycine, comprising 15 glycine units and 3 *l*-leucine units into a molecule of known structure and molecular weight 1213. Nine years later Emil Fischer's most gifted pupil Emil Abderhalden (1877-1950) similarly synthesized [Ber. *49,* 561-578 (1916)] an even larger (mol. wt. 1326) molecule comprising 19 amino acid units, viz. *l*-leucyl-triglycyl-*l*-leucyl-triglycyl-*l*-leucyl-triglycyl-*l*-leucyl-pentaglycyl-glycine. The technical details of the various synthetic methods cannot appropriately be considered here but comprise some of the most fundamental and generally useful tools of chemical and biochemical practice.

Emil Fischer's career was studded with evidences of the general recognition of his extraordinary powers and magnificent accomplishments. He received (1890) the Davy Medal of the Royal Society of London to which (1899) he was elected as a Foreign Member. He was elected foreign honorary member of the Chemical Society of London (1892) and of the Royal Institution of Great Britain (1904). He received (1902) the Nobel Prize for chemistry "in recognition of his special services in connection with his synthetic experiments in the sugar and purine groups of substances". This was the first Nobel Prize to an organic chemist, the initial issue (1901) having been assigned to Jacobus Henricus van't Hoff (1852-1911). It is perhaps significant of the magnitude of Fischer's work with the sugars that no other Nobel Prize was awarded for work in this field for thirty-five years, i.e. until 1937 in the case of Walter Norman Haworth (1883-1950).

In addition to the biographical material comprised in the periodical literature cited below, attention should be drawn to Fischer's autobiography "Aus Meinem Leben" published posthumously (1922) as a volume of 201 pages and three portraits by the press of J. Springer of Berlin.

P_1B Proc. Roy. Soc. (London) *98,* l-lvii (1921-22): P_1B J. Chem. Soc. *117,* 1157-1201 (1920) (Fischer Memorial Lecture; also reprinted in Chemical Society Memorial Lectures, Vol. 3 (1914-32), pp. 1-45, published by the Chemical Society of London, 1914): P_1B Ber. *54,* Special Number, 1-480 (1921) (often separately bound as a book biography): P_1B Naturwissenschaften *7,* 843-882 (1919): B Ber. *52-A,* 125-164 (1919); B Science *50,* 150-154, 346-347 (1919): B Nature *103,* 430-431 (1919): P_3B Chem. Ztg. *43,* 565-569 (1919): B Z. angew. Chem. *36,* 47-49 (1923): B Oesterr. Chem. Ztg. *22,* 173-175, 179-182 (1919)• B Z. physiol. Chem. *108,* 1-2 (1919): B J. Soc. Chem. Ind. *41,* 495-496 (R) (1922): P_2B J. Chem. Education *5,* 36-42 (1928); P_1B *ibid.*, *6,* 1190-1192 (1929).

FISCHER, (PHILIP) OTTO

Born Nov. 28, 1852, at Euskirchen, near Cologne, Germany; died April 4, 1932, at Erlangen, Germany, age 80.

This German organic chemist, born in the same city just 53 days later than his more famous cousin and professional colleague Emil Fischer (1852-1919), is furthermore not to be confused with Hermann Otto Laurenz Fischer (1888-), oldest son of Emil Fischer. After early schooling in the gymnasia of Bonn and of Aachen, Otto Fischer studied (1871) in Berlin under A. W. Hofmann (1818-1892), in Bonn (1872-74) under Friedrich Auguste Kekulé (1829-1896), and obtained his doctorate (1874) at the University of Strassbourg under A. von Baeyer (1835-1917) with a dissertation on the synthesis of hydrocarbons by condensation of aldehydes with toluene [Ber. 7, 1191-1197 (1874)]. After a brief postdoctoral sojourn at Berlin-Charlottenburg (1875) during which he collaborated with Carl T. Liebermann (1842-1914), he rejoined von Baeyer (who meantime had transferred to Munich) and spent there several extremely productive years (1878-84) as privat-dozent and assistant. Most of his life, however, was spent at the University of Erlangen where for forty years (1885-1925) until his retirement he was director of the chemical laboratory. When in 1897 he declined a call to succeed Theodor Curtius (1857-1928) at Kiel, Fischer was promised a new laboratory at Erlangen and this was soon afterward (1899-1900) constructed under his direction. It is of interest that the Erlanger chair had initially been accepted by Emil Fischer; when ill health prevented his actual transfer, however, Otto substituted for him. Upon Emil's recovery he accepted an invitation to Würzburg and Otto remained at Erlangen.

Otto Fischer is perhaps most famous for his part of the collaboration with Emil Fischer which resulted in the recognition of the first synthetic dyestuff, the Mauve or Parafuchsin of William Henry Perkin, Sr. (1838-1907), as a derivative of the hydrocarbon triphenylmethane. They showed that from the dyestuff by successive application of steps involving reduction, diazotization, and eventual replacement of the amino groups by hydrogen, triphenylmethane could be obtained. Conversely this hydrocarbon upon appropriate trinitration, reduction to the corresponding triamine, and suitable oxidation yielded the dyestuff pararosaniline. As a result of their work the constitution and relationships of this whole class of dyestuffs was firmly established. Otto Fischer was also the first (1877) to synthesize the important dyestuff Malachite Green (English Colour Index No. 657). His method, involving the condensation of benzaldehyde with *N*,*N*,-dimethylaniline, was soon afterward (1878) extended by Oskar Gustav Doebner (1850-1907), who substituted benzotrichloride for the benzaldehyde. Together with E. Hepp (1851-1917), Otto Fischer made extensive studies on the rearrangement of *N*-nitrosoamines to the corresponding nuclear nitrosated bases, and it is their names which are commemorated in the well-known Fischer-Hepp rearrangement.

B J. Soc. Dyers and Colourists *48,* 233 (1932) : B Nature *129,* 822 (1932) : B Chem. Ztg. *56,* 325 (1932) : B Ber. *65-A,* 78 (1932).

GADOLIN, JOHANN

Born June 5, 1760, at Åbo (also known as Turku), Finland; died August 15, 1852, at Wirmo, Finland, age 92.

This Finnish chemical pioneer and teacher sprang from a family long associated with learning and scholarship. His father Jacob Gadolin (1719-1802), at the time professor of physics at Åbo, then the capital of Finland and the seat of its university, subsequently became its professor of theology and eventually Bishop of Åbo. His maternal grandfather, Johann Brovallius (or sometimes Breval) (1705-1755), had also served at Åbo as professor of physics and likewise rose to the bishopric. After completing the high school of his native city, Johann Gadolin studied at the University under Pehr Adrian Gadd (1727-1797), the first occupant of its chair of chemistry, then spent four years in Sweden at the University of Uppsala under Tobern Olof Bergman (1735-1784), during which time he developed a brief but a close friendship with Carl Wilhelm Scheele (1742-1786), abruptly terminated by the death of the latter. Upon the death (1784) of Bergman, Gadolin was a candidate for the Uppsala post but, when Bergman's nephew Johann Arvidson Afzelius (1753-1837) was chosen, returned to Åbo as (1785) adjunct in chemistry, succeeding upon Gadd's death to the chair of chemistry which he graced (1797-1822) for 25 years until his retirement. He was succeeded by his most brilliant pupil Pehr Adolf von Bonnsdorf (1791-1839). Although Gadolin continued to conduct research for another five years, his scientific career was terminated (1827) by a great conflagration which practically destroyed Åbo and with it the university laboratories and collections. The university was afterward moved to Helsingfors (Helsinki) and Gadolin's remaining quarter century was passed in retirement in the country.

Gadolin participated actively in the conflict between the phlogiston theory and the new views of Lavoisier (1743-1794). At the time (1785) of joining the staff at Åbo, he adhered to the former, subsequently devised a theory of calcination which became a transitional philosophy, and eventually became not only a Lavoisier convert but published (1798) the first Swedish textbook embracing these views. On the practical side, Gadolin established the composition of Prussian Blue and suggested nearly forty years prior to the general introduction of volumetric methods by Gay-Lussac (1778-1850) a quantitative method for the determination of ferrous iron by titrating with standard ferricyanide.

Gadolin's most important chemical contribution, however, was doubtless his discovery (1794) of a rare earth element occurring as its oxide in a black mineral from Ytterby, Sweden. This mineral, now designated as *Gadolinite,* since has been found to have been a mixture of rare earths, but the principal element was later (1797) isolated by Anders Gustav Ekeberg (1767-1813) and called by him yttrium. Gadolin's name is also preserved in another rare earth element, discovered (1880) by Jean-Charles Galissard de Marignac (1817-1894) and named by Paul-Émile Lecoq de Boisbaudran (1838-1912) *gadolinium.* This is the only element named in honor of an individual man.

In honor of their distinguished countryman, the Society of Finnish Chemists (Suomalaisten Kemistien Seuran) established (1936) the Gadolin Fund for the stimulation and support of chemical research, together with the Gadolin Gold Medal for recognition primarily, but not necessarily exclusively, of the contributions of Finnish chemists. The first awards of this medal were made (1937) to Gustav Kommppa (1867-1949) and Ossian Aschan (1860-1939). [For pictures of both sides of the medal see Ind. Eng. Chem., News Ed. *16,* 585 (Nov. 10, 1938).] A handsome quarto biography of 389 pages by E. Hjelt (1855-1921) and R. Tigerstedt, entitled "Johan Gadolin: In Memoriam, etc." was published (1910) by S. Hirzel of Leipzig.

B Nature *86,* 48-49 (1911): BP_3 J. Chem. Education *9,* 1751-1752 (1932); *14,* 161-165 (1937): B Z. angew. Chem. *40,* 1431-1434 (1927): B Suomen Kemistilehti (Acta Chem. Fenn.) *6,* 107-112; *9A,* 25-27 (1936); *10A,* 106-109 (1937) (in Finnish).

GOOCH, FRANK AUSTIN

Born May 2, 1852, at Watertown, Massachusetts; died August 12, 1929, at New Haven, Connecticut, age 77.

This American pioneer in analytical chemistry was for three decades (1888-1918) identified with the Kent Chemical Laboratory of Yale University. After early education in Watertown and Cambridge, culminating in an A.B. (1872) from Harvard University, Gooch continued there into graduate study, meanwhile acting as assistant to Josiah P. Cooke (1827-1894). His residence was interrupted (1875-76) by mineralogical and crystallographic studies at the Imperial Museum in Vienna. This work, together with a thesis entitled "A Treatise on Crystallography on the Basis of Miller's System," led (1877) to the coupled Harvard degrees of A.M. and Ph.D. Gooch then again went to Europe with the intention of pursuing thermochemical studies at Copenhagen under Julius Thomsen (1826-1909), but when the latter did not encourage this program Gooch resolved his journey into a series of inspections of numerous European laboratories, which later proved advantageous during his supervision (1886-88) of the planning and construction of the Kent Chemical Laboratory at Yale. He next (1877-79) served at Harvard as private assistant to Wolcott Gibbs (1822-1908), an experience which was determinative in attracting Gooch toward devoting his life to the development of analytical chemistry.

During the next seven years, Gooch was concerned with analytical work in connection with the tenth U. S. Census (1879-81), the Northern Transcontinental Survey (1881-84), and the U. S. Geological Survey (1884-86). One permanently important result of this last connection was his examination of the thermal waters of Yellowstone Park [Bull. U.S. Geol. Survey, No. 47, 84 pp. (1888)]. Following these three connections, Gooch removed to New Haven to become professor of chemistry and director of the new Kent Chemical Laboratory (1886-1918), a connection which comprised his principal life work. He was elected (1897) to the National Academy.

Gooch's name is most familiar to contemporary chemists through the now world-famous Gooch filter. Developed during his work with Gibbs at Harvard [Proc. Am. Acad. Arts and Sci. *13,* 342-347 (1878); reprinted in Am. Chem. J. *1,* 317-323 (1879-80)], this was originally constructed of platinum and its transformation into the now more familiar porcelain came much later [cf. Z. anal. Chem. *64,* 224-227 (1924); *68,* 412-413 (1926)]. Less familiar are the numerous fundamental contributions which Gooch made to the methods of inorganic analytical chemistry, including the separation of lithium from other alkali metals, the determination of boric acid by distillation with methyl alcohol, his important studies on the necessary conditions for suitable precipitation of phosphates, and his extensive development of iodometric methods for the estimation of a wide variety of elements and radicals.

In addition to his research papers, Gooch was the author of several books, including those on "Inorganic Chemistry" (1905 with C. F. Walker), "Qualitative Analysis" (1906 with P. E. Browning), which passed through five additional revised and enlarged editions during Gooch's life, and "Quantitative Analysis" (1916). Besides these, he edited (1901) for the Yale Bicentennial series a two-volume collection of Kent Chemical Laboratory research papers and compiled (1912) a volume of "Methods of Quantitative Analysis originated or developed in the Kent Laboratory".

P_1B Biog. Memoirs Natl. Acad. Sci. U.S. *15,* 105-135 (1934) (incl. bibliog.): P_3B Ind. Eng. Chem. *15,* 1088-1099 (1923): B Am. J. Sci. (5) *18,* 539-540 (1929): B Proc. Am. Acad. Arts Sci. *70,* 541 (1934-5): B Natl. Cycl. Am. Biog. *12,* 329-330 (1904).

GUERICKE, OTTO VON

Born Nov. 20, 1602, in Magdeburg, Prussian Saxony, Germany; died May 11, 1686, at Hamburg, Germany, age 84.

This German natural philosopher was the inventor of the first vacuum pump and of the first machine for generating electricity by friction. As the descendant of a patrician Magdeburg family, he began (1617) the study of law at age 15, first at the University of Leipzig, then at Helmstedt and Jena; while concluding (1623) his professional preparation at the University of Leyden, Holland, he also interested himself in mathematics and mechanics. After travelling in France and England, he returned (1625) to Magdeburg and began to be active in civic affairs. He promptly (1627) became a member of the city government, and during the siege of Magdeburg (1631), in the course of the Thirty Years War (1618-48), led the attempts to defend the town. These, however, were unsuccessful; the community was almost completely destroyed and most of its inhabitants butchered. Guericke barely escaped with his life and for a time served as quartermaster general in the service of Gustavus Adolphus. Because of his training and ability he was eventually placed in charge of the reconstruction of the city, of which (1646) he became burgomeister (mayor). Although during the next fifteen years he was almost continuously occupied with municipal business, he nevertheless found time for scientific experimentation. His later years were clouded by deplorable ingratitude on the part of his fellow citizens, and finally, at the age of 79, driven out (1681) by the plague, Guericke fled to Hamburg, where he died at the home of his son.

Guericke's strong interest in science, possibly stimulated by reports of the discoveries of Galileo Galilei (1564-1642), Evangelista Torricelli (1608-1647), and Blaise Pascal (1623-1662), turned initially to the study of pneumatics and specifically toward attempts to create empty space. After many abortive attempts to pump out water from a filled container, he finally tried omitting the liquid and was promptly successful in removing the air directly. He constructed (1650) several true air-pumps, each comprising a cylinder and piston with appropriate valves and operating under a water seal. Although these instruments were not themselves described by their inventor, an account of one of them was given (with Guericke's consent) by Kaspar Schott in his ''Mechanica hydraulico-pneumatica'' of 1657. Substantial modifications and improvements were introduced by the English scientists Robert Boyle (1627-1691) and Robert Hooke (1635-1703). Still further refinements were effected (1709) by Francis Hauksbee (the elder) whose mechanism has been preserved in the collection of the Royal Society of London. These pioneer pumps served during the 18th century but were gradually displaced during the 19th, first by oil-filled, later by mercury-filled machines. Early in the 20th century, rotary pumps were introduced, followed by various modern types of vapor condensation devices.

More important than the mere invention of Guericke's air-pump were the experiments which it made possible for him to execute. These he eventually recorded (1663) in an epochal Latin work entitled "Experimenta nova, ut vocant, Magdeburgia de vacuo spatio", subsequently (1672) printed at Amsterdam. A copy of this very rare item is owned by McGill University, Montreal, Canada. The undertaking actually comprised seven books, of which the third and most important was translated (1894) into German and is now generally accessible as No. 59 of the series by Wilhelm Ostwald (1853-1932) entitled "Klassiker der Exacten Wissenschaften". Guericke showed that light but not sound could be transmitted through a vacuum. He determined directly the specific gravity of air by comparing the weights of the same container when evacuated and when filled with air. He recognized the property of every mass of air to expand throughout all available space. He noted the energy with which equalization of pressure took place and inferred that winds were caused by differences in atmospheric pressure. He was impressed by the hard clash (water hammer effect) produced by two masses of water meeting in an evacuated space. Through these and many other experiments Guericke was first to demonstrate that air was a tangible material which at will could be introduced into and removed from any given space.

Guericke's air-pump was dramatized by its employment in his famous demonstrations (1654) of the Magdeburg spheres. Two copper hemispheres about 22 inches in diameter were placed together to form a sphere, the joint being sealed by an oil-impregnated leather ring. After evacuation of the assembled vessel, it was found that the halves strongly resisted separation even under the force of two oppositely directed teams of eight horses each. On the other hand, after readmission of air to the evacuated space the two hemispheres separated spontaneously. Subsequently, this experiment was repeated, using even larger hemispheres and two teams of twelve horses each.

Otto von Guericke also devised (1650) the first electric machine or electrostatic generator. This consisted of a large ball of sulfur mounted on an iron axis so as to be revolved and rubbed by the hand. With this primitive device, he was able to demonstrate in greater detail the phenomenon of electrostatic repulsion first reported (1629) by the Italian Jesuit Nicolaus Cabaeus, and was first to discover induction, conduction, discharge by proximity to flame, and emission of luminous glow.

Soon afterward (1675) Isaac Newton (1643-1727) substituted a glass globe for the ball of sulfur, but practical utilization of the electrical charges developed had to await the development (1745) of the Leyden jar by Peter van Musschenbroeck (1692-1761).

The only detailed biography of von Guericke derived from original sources is that by F. W. Hoffmann (Magdeburg 1874). However, the first of the references cited below, prepared in commemoration of the 250th anniversary of Guericke's death, is especially valuable and includes many pictures.

P_1B Z. tech. Physik. *17,* 209-218; 345-354 (1936) : P_1B Physik. Z. *37,* 771-780 (1936) (identical with second section of preceding reference) : B Naturwissenschaften *24,* 305-309 (1936) : B Radiography *2,* 6-8 (1936) : B Chem. Apparatur *23,* 73-76, 89-92 (1936) : B Umschau *40,* 367-370 (1936) : B Z. Ver. deut. Ing. *80,* 557-562 (1936).

HESS, GERMAIN HENRI

Born Aug. 7, 1802, in Geneva, Switzerland; died Dec. 12, 1850, in St. Petersburgh, Russia, age 48.

Hess, one of the foremost physical chemists of Russia during the early 19th century, became (1832) professor of chemistry and inspector of mining cadet corps, one of the founders (1828) of the Technological Institute of St. Petersburgh, and a member (1834) of the Russian Academy of Science. His textbook, "Fundamentals of Pure Chemistry" (1831), passed through seven editions (last in 1849). With the collaboration of P. G. Sobolevskij, S. J. Nečaev, and M. S. Soloviev, he devised a rational Russian chemical nomenclature still employed. Internationally, he is best known for his work (1836-41) on heats of chemical reactions, compactly assembled (1890) by W. Ostwald as the ninth of his famous series "Classics of the Exact Sciences", and familiar to chemists as Hess' law of constant heat summation.

P_3B J. Chem. Education *11,* 228 (1934); *28,* 581-583 (1951).

VAN 'T HOFF, JACOBUS HENRICUS

Born Aug. 30, 1852, at Rotterdam, Holland; died March 1, 1911, at Steglitz, Berlin, Germany, age 59.

Destined to become one of the greatest of all physical chemists, this Dutch scientist, son of a Rotterdam physician, began his professional training with two years (1869-71) at the Delft Polytechnicum, and continued it with a year (1871-72) at that oldest and most famous center of Dutch education, the University of Leyden. At the age of 20 van't Hoff next spent a year (1872-73) at the University of Bonn, where the influence of the great Fredrich Auguste Kekulé (1829-1896) was soon to have an important bearing upon his own scientific contributions. The following year (1873-74) van't Hoff spent in Paris at the École de Médecine under Charles Adolphe Wurtz (1817-1884), where he first met the fellow student Joseph Achille Le Bel (1847-1930) whose name was afterward to be so closely associated with his own. Van't Hoff's period of preparation for a scientific career was concluded (1873-74) at the Dutch University of Utrecht where, under Gerardus Johannes Mulder (1802-1880), he carried out a research on cyanoacetic and malonic acids and on Dec. 22, 1874, obtained his doctorate.

Despite the recognition which ultimately he was to receive, the 22-year-old Dr. van't Hoff experienced great difficulty in finding the first rung of his academic ladder. After a year of fruitless effort, he obtained (1876) an appointment as privat dozent at the Royal Veterinary School at Utrecht. In the following year (1877), however, when the ancient Amsterdam Athenaeum became the University of Amsterdam, van't Hoff obtained a post as lecturer. After shortly (1878) being designated as professor of chemistry, mineralogy, and geology, van't Hoff remained as such for 18 years (1878-96), declining (1887) a flattering call from the University of Leipzig to the newly created chair of physical chemistry which was eventually filled by Wilhelm Ostwald (1853-1932). At this juncture, however, van't Hoff accepted an appointment to the Prussian Academy of Sciences at Berlin, Germany, where he was provided with ample laboratory facilities, relieved of all administrative responsibilities, and expected only as an honorary professor of the University of Berlin to give one lecture a week. In this post van't Hoff spent the last 15 years (1896-1911) of his life despite the fact that withdrawal of so distinguished a native son aroused considerable criticism in Holland. [For numerous examples of the biting cartoons stimulated by the circumstances of his withdrawal from Holland see their reproductions in Ind. Eng. Chem. *15*, 192-193 (1923).]

Since the major scientific contributions of van't Hoff may be classified into four principal categories—stereochemistry, chemical equilibrium, theory of solutions, and the physical chemistry of the Stassfurt salt deposits—brief comments upon each of these aspects may be useful in outlining the nature of his interests.

Only a week after his 22nd birthday and several months before obtaining his doctorate, van't Hoff laid a secure foundation for his subsequent reputation by the publication on Sept. 5, 1874, of a small (11 pp.) pamphlet in Dutch, the translation of whose title reads "An attempt to extend to space the present structural chemical formulae, with an observation on the relation between optical activity and the chemical constitution of organic compounds". Here he stated fully all the fundamental premises of modern stereochemical theory, both optical and geometrical. Two months later Joseph Achille LeBel (1874-1930) entirely independently set forth essentially the same ideas in France [Bull. soc. chim. (2) *22,* 337-347 (1874)]. Despite the fortuitous fact that the two authors had the previous year been classmates in Wurtz's Paris laboratory, neither knew of the stereochemical views of the other, and ever since their independent publications their names have been happily and indissolubly associated. Their great contribution to stereochemistry was to define the structural conditions for asymmetry and to deliver the principle as an effective instrument for the service of their fellow workers. In the following year (1875) van't Hoff's initial Dutch pamphlet was enlarged and translated into the 44-page "La chimie dans l'Espace" (2nd ed. 1887, 3rd. ed. 1892) and a corresponding German version with a foreword by Johannes Wislicenus (1835-1902) soon (1877) appeared as "Die Lagerung der Atome im Raume" (2nd ed. 1894, 3rd ed. 1908). Although his contribution to stereochemistry eventually received the full recognition which it deserved, it initially was received with indifference and by some, including A. W. Hermann Kolbe (1818-1884), with open sarcastic opposition [see J. Soc. Chem. Ind. *43,* 1048-1050 (1924)] and it is perhaps indicative of van't Hoff's good judgment that he had not allowed his natural enthusiasm for his project to lead him to offer it for his doctoral dissertation. [For those wishing to survey more fully the details, circumstances, and consequences of the van't Hoff-LeBel concepts, reference is made to an extensive survey by Ernst J. Cohen (1869-1941) published in Dutch [Chem. Weekblad *21,* 482-501 (1924)] and to two reviews in German by Paul Walden (1863-) [Chem. Weekblad *22,* 34-61 (1925); Z. angew. Chem. *38,* 429-439 (1925)]].

After occupying (1878) the chair at Amsterdam, van't Hoff's attention turned from organic chemistry to the study of reaction velocities and to the conditions affecting chemical equilibrium. Building upon the foundation laid by the Norwegian mathematical physicist Cato Maximillan Guldberg (1836-1902) and his son-in-law the Norwegian chemist Peter Waage (1833-1900), van't Hoff developed, extended, systematized, coordinated and applied both old information and the results of his own new experiments into a powerful new approach. For example, he first classified reactions into unimolecular, bimolecular, etc., according to the number of participating molecules. He devised new methods for determining the order of a reaction. He pioneered the study of chemical kinetics and the application of thermodynamics to chemical equilibria. He introduced the now well-known symbol $\rightleftharpoons$ for reversible reactions and the term "transition point". He deduced the relationship between equilibrium constant and temperature. He recognized the law of mobile equilibrium, a special case of the more general principle developed in the same year (1884) by Henry Louis Le Chatelier (1850-1936). Much of this progress was collected (1884) in his second epochal book entitled "Études de dynamique chimique", subsequently translated (1896) to both English and German.

The third aspect of van't Hoff's great contribution, his extensive studies on solutions, was a direct outgrowth of the second and indeed more or less overlaps it. He was the first to show (1887) that in a solution of a non-electrolyte the resultant depression of the freezing point and raising of the boiling point is proportional to the number of dissolved molecules. He was first to recognize that the osmotic pressure of such a solution varies with concentration and temperature exactly as does the pressure of an ideal gas and first formulated what has since been called the van't Hoff equation. To represent the colligative properties of electrolytes by means of the relation for non-electrolytes, van't Hoff suggested a factor i, the necessity for which was soon accounted for in the electrolytic dissociation theory of Svante Arrhenius (1859-1927) [not to be confused with Axel Arrhenius (1757-1824), the discoverer of gadolinite]. He was the first [Z. physik. Chem. *5*, 322-339 (1890)] to conceive of solid solutions and to study their properties.

Although his contributions to stereochemistry, reaction kinetics and equilibrium, and the theory of solutions would more than have sufficed to establish van't Hoff as the chief founder of modern physical chemistry, he added to them after his removal

(1896) from Amsterdam to Berlin, a fourth epochal contribution in his studies of oceanic salt deposits with particular reference to those at Stassfurt, Germany. In this first and still greatest application of physical chemistry to geological problems, he was notably assisted for a decade by Wilhelm Meyerhoffer (1864-1906), a friend and former pupil who had accompanied his chief to the German capital. The results of their joint investigations, initially recorded in 51 separate papers published between 1897-1906 in the Proceedings of the Berlin Academy of Sciences, have been collected into two volumes (1905, 1909) under the title "Zur Bildung der Ozeanischen Salzablagerungen".

In addition to the scientific papers and books recording the results of his own researches, it should be noted that van't Hoff joined with Wilhelm Ostwald (1853-1932) in establishing (1887) the important *Zeitschrift fur physikalischen Chemie,* of which the two scientists were joint editors continuously through the first 75 volumes until van't Hoff's death (1911). It is also noteworthy that the initial volume of this journal included both van't Hoff's first paper on the role of osmotic pressure in solutions [Z. physik. Chem. *1,* 481-508 (1887)] and that of S. Arrhenius on electrolytic dissociation [Z. physik. Chem. *1,* 631-648 (1887)]. In honor of the 25th anniversary of his doctorate, Vol. 31 (1899) of this journal was dedicated to van't Hoff.

Of the honors bestowed upon van't Hoff, only a few may be mentioned here. He was named an honorary member of the chemical societies of London (1888), Germany (1889), and America (1898). He was elected a foreign member of the American Academy of Arts and Sciences (1895), and after receiving jointly with LeBel the Davy Medal (1893) of the Royal Society of London was elected (1897) to foreign membership. After receiving (1901) the first Nobel Prize in Chemistry ("for his services in connection with the discovery of the laws of chemical dynamics and osmotic pressure in solutions"), he was elected a foreign associate of the U. S. National Academy of Sciences (1901) and of the French Academy of Sciences (1905). He visited the United States in 1901 and an informal account of his experiences [Sci. Monthly *7,* 563-568 (1918)] still possesses interest.

In addition to the numerous citations from the periodical literature, attention should be drawn to two book biographies. Both of these were written by van't Hoff's Amsterdam colleague, the skilled chemical historian Ernst J. Cohen (1869-1941) and are published in German. The older and shorter is the 56-page brochure published (1899) on the occasion of van't Hoff's doctoral jubilee by the Verlag von Wilhelm Engelmann in Leipzig; the later very extensive (688 pp.) biography appeared (1912) under the title "Jacobus Henricus van't Hoff, Sein Leben und Werken" from the Akademische Verlagsgesellschaft of Leipzig.

P_1B J. Chem. Soc. *103,* 1127-1143 (1913) (van't Hoff Memorial Lecture; also reprinted in Chemical Society Memorial Lectures, Vol. II (1901-1913) pp. 255-271, published by The Chemical Society, London, (1914): P_1B Proc. Roy. Soc. (London) *86A,* xxxix-xliii (1911-12): P_1B Ber. *44,* 2217-2252 (1911): P_1B Z. physik. Chem. *76,* 641-642 (1911); P_1B *ibid., 31,* frontispiece, v-xxv (1899); P_1 *ibid., 50,* frontispiece (1904): B Smithsonian Institution Annual Reports for 1913, 767-789 (1914): P_1B Z. Elektrochem. *16,* 857-865 (1910); P *ibid., 17,* 210-211 (1911); B *ibid., 10,* 479-481 (1904): P_1B J. Chem. Education *6,* 826-829 (1929); *ibid., 7,* 2697-2700 (1930); $3P_2$ (informal) *ibid., 11,* 355-359 (1934); P_3B *ibid., 25,* 303 (1948): B Z. angew. Chem. *24,* 1074-1087 (1911): P_3B Chem. Ztg. *35,* 289-290 (1911): P_3B Oesterr. Chem. Ztg. *34,* 54-59 (1931): B Nature *61,* 321-322 (1900); *86,* 84-86 (1911): B Science *11,* 241-246 (1900): B J. chim. phys. *9,* 503-504 (1911): P_2B Pop. Sci. Monthly *79,* 94-97 (1911): P_1B Proc. Am. Phil. Soc. *50,* iii-xii (1911): B J. Soc. Chem. Ind. *30,* 405-406 (1911): B Chem. Weekblad *1,* 511-517 (1904); *8,* 197-198, 228-238; (P_1) 322-329 (1911); ($2P_3$) *21,* 482-501 (1924); ($3P_3$) *31,* 782-788 (1934); (P_3) *37,* 494-498 (1940); *38,* 116-117 (1941); *44,* 474-477 (1948) (all in Dutch).

KINGZETT, CHARLES THOMAS

Born Nov. 19, 1852, at Oxford, England; died July 29, 1935, at West Byfleet, Surrey, England, age 83.

This British chemist, technologist and author studied chemistry at Oxford University at the age of 14 under Sir Benjamin Collins Brodie (1817-1880) and A. G. Vernon Harcourt (1834-1919). His industrial chemical experience began in his association (1870-72) with Walter Weldon (1832-1885) in the study of manufacturing processes for chlorine and the recovery of sulfur from alkali waste, and (1872-77) as chief chemist of Snape's Soda Works in Liverpool. For a short period (1877-79), he operated (with B. H. Paul) a consulting practice in London. An investigation of the antiseptic properties of conifer essential oils and their oxidation products led to his establishment (1878) of the Sanitas Company, Ltd., of which he was initially chemist and technical manager, later chairman, resigning in 1926.

Kingzett's first paper [Proc. Chem. Soc. *1873,* 456] was on the formation of sodium sulfide by action of hydrogen sulfide on sodium chloride. He was first [J. Chem. Soc. *13,* 405-410 (1875)] to isolate calcium hypochlorite in crystalline form. He first [J. Chem. Soc. *37,* 792-807 (1880)] established the fact that when phosphorus is oxidized by air or oxygen while partially submerged in water not only is ozone produced (as was previously supposed) but also hydrogen peroxide.

Kingzett was a charter member (1877) of the (British) Institute of Chemistry and (1881) of the (British) Society of Chemical Industry. He served (1885-6) as vice president of the Society of Public Analysts. His several books include ''The History, Products and Processes of the Alkali Trade'' (1877), ''Animal Chemistry, or the Relations of Chemistry to Physiology and Pathology'', and particularly Kingzett's ''Chemical Encyclopedia'' (1st ed. 1919, 6th ed. 1940, revised and edited by R. K. Strong).

B J. Chem. Soc. *1935,* 1899-1902: B Analyst *60,* 649 (1935): P_3B J. Soc. Chem. Ind., Special Jubilee No., page 116 (1931).

Legendre, Adrien Marie

Born Sept. 18, 1752, at Toulouse, France; died Jan. 10, 1833, at Paris, France, age 81.

This French mathematician comprised one of the illustrious trio which included also J. L. Lagrange (1736-1813) and P. S. Laplace (1749-1827). Although Legendre was the youngest, all three were contemporaneously at the height of their powers for a period of some four decades (1773-1813). The subject of this digest is not to be confused with Louis Legendre (1752-1797), the French revolutionist, although the latter was born in the same year.

Subsequent to completion of his education at the Collège Mazarin in Paris, A. M. Legendre held for five years (1780-85) a professorship at the École Militaire. During this period he won (1782) at the age of 30 a prize of the Berlin Academy for a memoir on the trajectory of a projectile and another (1783) of the Paris Academy for an analysis of the attraction of planetary spheroids. These early evidences of mathematical genius attracted such favorable attention that he was elected (1783) to membership in the French Academy of Sciences, succeeding to the chair of d'Alembert (1717-1783). Shortly thereafter (1789) he was also elected a foreign member of the Royal Society of London at the same meeting which conferred similar honors upon the chemist C. L. Berthollet (1748-1822), his fellow mathematician P. S. Laplace (1749-1827), and both P. F. A. Méchain (1744-1804) and J. D. Cassini (1748-1845), with whom Legendre was currently (1787-89) associated in operations for the junctions of the meridians of Paris and London. Legendre was also appointed (1795) professor at the École Normale and served as a member of various governmental commissions. Later (1824) at the age of 72, however, he declined to bow to government dictation of the Academy, was deprived of his pension, and spent the last decade of his life in poverty.

Legendre's "Élements de Géométrie" (1794), in which he rearranged the propositions of Euclid, eventually appeared in many editions, was translated into almost every European language, and is even now regarded as one of the most successful textbooks ever written. His "Théorie des nombres" (1798), which after a second edition (1808) and two supplements (1816, 1825) appeared still later (1830) as a third edition in two volumes, contained Legendre's law of quadratic reciprocity. This principle, which he had first enunciated much earlier (1785), has been characterized both as "the gem of arithmetic" and the most important general mathematical discovery since P. Fermat (1601-1665) in the science of numbers. Legendre's "Exercices de calcul intégral" (1811, 1816, 1817) contained in its third volume elaborate tables of elliptic integrals together with an account of the method by which they were constructed by the author. Although he had invented these functions and had alone pursued the subject for some 40 years, this treatment had scarcely appeared when the independent work of N. H. Abel (1802-1829) in Norway and K. G. J. Jacobi (1804-1851) in Germany revolutionized it completely. Legendre's last great treatise was the "Traité des fonctions elliptiques, etc." (2 volumes, 1825, 1826, 3rd volume 1833).

Legendre formulated (1806) the first satisfactory treatment of the method of least squares and invented spherical harmonic analysis. His name is still associated with the Legendre coefficients (or functions) of the latter and with the Legendre theorem of spherical trigonometry. He was a member of a commission nominated for the introduction of the metric system in France; [for a detailed account of his participation in the reformation of weights and measures, see Osiris *1,* 314-340 (1936)].

B Proc. Roy. Soc. (London) *3,* 230-232 (1830-7): B Smithsonian Institution, *Ann. Rept. for 1867,* 131-157 (1872).

MAGNUS, HEINRICH GUSTAV

Born May 2, 1802, in Berlin, Germany; died April 4, 1870, at Berlin, Germany, age 68.

This German scientist was almost equally chemist and physicist. In addition to important research contributions, he also demonstrated exceptional talents in the teaching of both disciplines. He came from a wealthy and talented family of which he was the only scientist, although one of his five brothers, Eduard Magnus (1799-1872), became a celebrated historical painter. After five years at the University of Berlin culminated in his doctorate, H. G. Magnus spent a year (1828) at Stockholm under Jöns Jacob Berzelius (1779-1848), followed by another (1829)in Paris with Joseph Louis Gay-Lussac (1778-1850) and Louis Jacques Thenard (1777-1857). He then returned to the University of Berlin, where, after qualifying (1831) as lecturer in technology and physics, he taught with exceptional brilliance for nearly forty years (1831-70). He rose through the ranks of extraordinary professor (1834) and ordinary professor (1845) to be thrice (1847, 1858, 1863) elected dean of the faculty and to serve (1861) as "rector magnificus". Upon his death, his chair at Berlin was taken over by Hermann Ludwig Ferdinand von Helmholtz (1821-1894).

During the early part of his Berlin career, Magnus also lectured on chemistry at the Berlin Gewerbeschule in the absence of Friedrich Wöhler (1800-1882), and on physics (1832-40) at the Gewerbe-Institut. He was a member of the juries of the London Exhibitions of 1851 and 1862 and of the Paris Exhibitions of 1855 and 1867, and represented Prussia at the Frankfort Conference (1865) on weights and measures which led to the adoption of the metric system throughout Germany. He was instrumental in the founding (1867) of the German Chemical Society and served it as a vice president. He was elected (1863) a foreign member of the Royal Society of London.

Magnus' researches extended over a period of 45 years. His first memoir, published [Poggendorf's Ann. Physik *3,* 81-88

(1825); Ann. chim. phys. (2) *30,* 103-109 (1825)] at the age of 23 while he was still a student, comprised the discovery of the preparation of pyrophoric iron, cobalt and nickel by reduction of their oxides in hydrogen. While working with Berzelius, he discovered [Poggendorf's Ann. Physik *14,* 239-242 (1828); Ann. chim. phys. (2) *40,* 110-111 (1829)] the first of the platino-ammonia complexes (now known as ammines), viz., $PtCl_2(NH_3)_2$, still known as "Magnus' green salt". Soon afterward, together with C. F. Ammermuller, Magnus reported [Poggendorf's Ann. Physik (2) *28,* 514-525 (1833); Ann. chim. phys. (2) *53,* 92-105 (1833); (Liebig's) Ann. *11,* 18-24 (1834)] the discovery of periodic acid and studied certain of its salts. Organic chemistry is indebted to Magnus for his discovery [Poggendorf's Ann. Physik *27,* 367-387 (1833); Ann. chim. phys. (2) *52,* 139-160 (1833); (Liebig's) Ann. *6,* 152-173 (1833) of the three closely related compounds ethyl hydrogen sulfate ("sulfovinic acid") [Beil. I-325], aethionic acid (β-sulfoethyl hydrogen sulfate) [Beil. IV-15], and isaethionic acid (β-hydroxyethanesulfonic acid) [Beil. IV-13].

Magnus also published on a wide variety of topics in physics. Among these may be mentioned here studies of the absorption of gases in the blood (1837-45), the thermal expansion of gases (1841-44), vapor pressure of water and of aqueous solutions (1844-54), thermoelectricity (1851), electrolysis (1856), thermal conductivity of gases (1860) and the polarization of heat (infra-red radiation) (1866-68).

An important and detailed account of Magnus' life and work in the form of a memorial lecture by August Wilhelm von Hofmann (1818-1892) to the German Chemical Society on December 14, 1870 [and often erroneously cited as published in Ber. *3,* 993 ff. (1870)] is actually printed with portrait in Hofmann's classic work entitled "Zur Erinnerung an Vorangegangene Freunde", Vol. I, pp. 45-194 (1888).

B Proc. Roy. Soc. (London) *20,* xxvii-xxix (1871-72): B J. Chem. Soc. *24,* (New Series 9), 610-615 (1871): B Nature *1,* 607 (1870); BP_3 *2,* 143-145 (1870): B Chem. News *21,* 201 (1870): B Smithsonian Institution Annual Report for *1870,* 223-230 (1872): B Ber. *3,* 331-333, 984 (1870): B Moniteur scientif. *12,* 657-660 (1870): B Archives Sci. Phys. Nat. *40,* 61-76 (1871): B Berlin Akad. Abhandl. *1871,* 1-17: B Poggendorf's Ann. Physik (5) *19,* 676 (1870).

MICHELSON, ALBERT ABRAHAM

Born Dec. 19, 1852, at Strelno, then in Germany near the Polish border; died May 9, 1931, at Pasadena, California, age 79.

This great American physicist, the son of Jewish parents who emigrated (1854) to the United States while he was still an infant, obtained his early education in the public schools of San Francisco and of Virginia City, Nevada. Pursuant to the father's desire that his son should enter the American navy, young Michelson took at age 17 the examinations for a congressional appointment to the U. S. Naval Academy at Annapolis. When the coveted place was awarded to a competitor who had equalled Michelson's results, however, he secured a personal conference in Washington with President Grant with a view to obtaining one of the ten Presidential appointments-at-large. When these proved to be otherwise committed, Michelson continued to Annapolis in the hope that a vacancy might develop through some appointee's failure in the physical examination. After waiting three days even this possibility faded and he had just started back to Washington for a second interview with President Grant when he was overtaken by a messenger bearing the news that he had been named as an eleventh appointee-at-large. In view of this unconventional circumstance, Michelson always maintained that his career had been made possible by an illegal though Presidential action.

After an undistinguished career (1869-73) at Annapolis, followed by a period of active duty at sea, Michelson returned to the Academy for service (1876-77) as instructor in physics and chemistry and later (1877-79) as professor of physics. At this juncture, he spent a year (1879-80) in the Washington office of the Nautical Almanac, during which he collaborated with its Director, Simon Newcomb (1835-1909). Accompanied by his wife and their two young children, Michelson spent the next several years (1880-83) in Europe, studying successively at Berlin in the laboratory of H. L. F. von Helmholtz (1821-1894), in Heidelberg with G. H. Quincke (1834-1924), and finally in Paris in the laboratories of A. M. Cornu (1841-1902), E. Mascart (1837-1908), and G. Lippmann (1845-1921). Upon returning from Europe, Michelson then entered upon the academic phase of the professional career which was to see his association with three American universities. This began at the Case School of Applied Science in Cleveland, Ohio, where for six years (1883-89) as professor of physics he was associated with Edward W. Morley (1838-1923) and carried out the famous Michelson-Morley experiment. For a shorter period (1889-92), he held a similar post at Clark University in Worcester, Massachusetts. His principal academic association, however, was with the University of Chicago, of whose department of physics he was the first head (1892-1925). When at the age of 73 he was succeeded by his friend and collaborator Robert A. Millikan (1868-   ), Michelson served four years (1925-29) in the first of the University's Distinguished Service Professorships. During his last years (1929-31), he held the title of research associate of the Carnegie Institution of Washington and carried on his work at Pasadena, California.

Michelson's entire life work was concerned with light and optics, particularly with reference to increasing the precision of measurements in this field. His first professional paper, published [Am. J. Sci. (3) *15*, 394-395 (1878)] at age 26 while he was still a student at Annapolis, bears the title "On a Method of Measuring the Velocity of Light"; his last, upon which he was at work only two days before his death and published [Astrophys. J. *82*, 26-61 (1935)] posthumously, has the title "Measurement of the Velocity of Light in a Partial Vacuum".

Although the principal theme of his life work for more than half a century was the measurement of the speed of light with ever-increasing precision, many variations and embellishments of this theme also resulted in epochal scientific advances. Before he was 30 he devised that extraordinary and versatile instrument known as the Michelson interferometer, whose further development and application comprised so large a part of his life work. His first use of this instrument for testing the relative velocity of the earth and the ether [Am. J. Sci. (3) *22*, 120-129 (1881); also reprinted in German in Naturwissenschaften *19*, 779-784 (1931)] culminated (1886-87) in the classical Michelson-Morley experiment [Am. J. Sci. (3) *34*, 333-345 (1887); Phil. Mag. (5) *24*, 449-463 (1887)], now recognized as one of the most fundamentally significant contributions to science since it provided the basis for the special (and later the general) Theory of Relativity of Albert Einstein (1879-   ).

Beginning in the nineties, Michelson devoted special effort to the attainment of very high spectroscopic resolution, particularly by means of ruled gratings. This work led to the development of the echelon spectroscope, the preliminary announcement of which [Am. J. Sci. (4) *5*, 215-217 (1898)] was quickly followed by full descriptions [Astrophys. J. *8*, 37-47 (1898); Proc. Am.

Acad. Arts and Sci. *35,* 111-119 (1899-1900)]. About this same time, in collaboration with Samuel Wesley Stratton (1861-1931) (later head (1901-23) of the U. S. National Bureau of Standards and President (1923-30) of the Massachusetts Institute of Technology), Michelson developed a new harmonic analyzer [Am. J. Sci. (4) *5,* 1-13 (1898); Phil. Mag. (5) *45,* 85-91 (1898)]. By application of interferometric methods, Michelson (in collaboration with H. G. Gale) made the first accurate measurements of the rigidity and viscosity of the earth [Astrophys. J. *50,* 330-345 (1919); J. Geol. *27,* 585-601 (1919)]. Another application of his optical methods to cosmic problems in the determination of the diameter of Betelgeuse as 240,000,000 miles [Astrophys. J. *53,* 249-259 (1921) with brief version in Proc. U. S. Natl. Acad. Sci. *7,* 143-146 (1921)] represented the first approximately accurate determination of the size of a star.

Despite the fact that most of Michelson's scientific career was spent in academic laboratories, his views on research collaboration with students may have some interest to current workers. After his first decade at Chicago, he gave up completely the conventional association with graduate students and relied exclusively on paid assistants. He is reliably quoted as complaining that "graduate students . . . either spoil the problem for me because they haven't the capacity to handle it as I want it handled and yet they make it impossible for me to discharge them and do the problem myself; or else . . . they get good results and at once begin to think the problem is theirs instead of mine, when in fact the knowing of what kind of a problem it is worth while to attack is in general more important than the mere carrying out of the necessary steps". He took no part in general university administrative or educational problems outside the department of physics.

Although Michelson never received any of the conventional academic degrees as the culmination of any specified program of study, his distinguished scientific accomplishments were recognized by 11 honorary degrees and countless other awards. He was elected (1902) as a foreign member of the Royal Society of London and later (1907) received its highest award, the Copley Medal. In that same year (1907), he was awarded the Nobel Prize in Physics, the first American to receive Nobel recognition in any scientific field. Other notable medals received by Michelson include the Elliott Cresson Medal (1912) and the Franklin Medal (1923) of the Franklin Institute of Philadelphia, the Draper Medal (1916) of the U. S. National

Academy of Sciences, the Gold Medal (1923) of the Royal Astronomical Society, and the Duddell Medal (1930) of the Physical Society of London. Recognition in election to distinguished offices in professional societies includes his service as President of the American Physical Society (1901-3), President of the American Association for the Advancement of Science (1910-11), and President (1923-27) of the U. S. National Academy of Sciences to which he had been elected (1888) at the age of 36.

P_1B Biog. Memoirs Natl. Acad. Sci. U. S. *19,* 121-146 (1938) (incl. bibliog.) : P_1B Obit. Notices, Fellows Roy. Soc. (London) *1,* 18-25 (1932) : 2P_1B J. Opt. Soc. Am. *18,* 147-154 (1929) (incl. bibliog.) : P_1B Science *73,* 549-550 (1931) ; reprinted identically in Phys. Rev. (2) *37,* 1377-1379 (1931) : B Science *69,* 481-485 (1929) : P_2 Sci. Monthly *32,* 566 (1931) ; 2P_2 *ibid. 27,* 566-571 (1928) ; P_2 *ibid. 23,* 567 (1926) : B Proc. Am. Acad. Arts and Sci. *71,* 530-532 (1935-36) : P_1B Nature *117,* 1-6 (1926) : B *ibid. 127,* 751-753, 825-826 (1931) : P_1B J. Chem. Education *8,* 2108-2110 (1931) : B Naturwissenschaften *19,* 777-779 (1931) : B Dict. Am. Biography *12,* 593-596 (1933) : B Natl. Cycl. Am. Biog. P_1B Current Vol. *C,* 42-43 (1930) ; P_1B *33,* 511-513 (1947) ; B *12,* 100 (1904).

Moissan, (Ferdinand Fréderic) Henri

Born Sept. 28, 1852, at 5 Rue Montholon, Paris, France; died Feb. 20, 1907, in Paris, France, age 55.

This French master of inorganic chemistry received his early education in the municipal college of Meaux in the Department of Seine-et-Marne. After two years (1870-72) as an apothecary's apprentice in Paris, he spent a year (1872-73) at the Musée d'Histoire Naturelle studying under Edmond Frémy (1814-1894) and attending the courses of H. St. Claire-Deville (1818-1881), J. Henri Debray (1827-1888) (in whose laboratory he later isolated fluorine), and others. He then transferred to the neighboring laboratory of P. P. Dehérain (1830-1902) in the École Pratique des Hautes Études, with which he was associated for six years (1873-79), during which time he obtained various degrees and published as his first paper and only work outside the field of inorganic chemistry a small contribution on the respiration of plants in the dark. After serving a year (1879-80) as instructor at the Agronomic Institute and obtaining his doctorate (1880) from the University of Paris, he became associated with the École Supérieure de Pharmacie, first (1880-83) as senior demonstrator and later after the death of Jules B. Bouis (1822-1886) as Professor of Toxicology, a chair which he retained (1886-99) for 13 years. Although his sole research interest had been confined to the field of inorganic chemistry for two decades, he first delivered formal lectures in that subject at the École de Pharmacie in 1899. Upon the retirement of Louis J. Troost (1825-1911), Moissan succeeded him in the chair of Inorganic Chemistry in the Faculty of Sciences of the University of Paris (the Sorbonne) and held this post (1900-07) during the brief remaining portion of his life. After Moissan's happy marriage at age 30 (1882) his wife's father, who was extremely sympathetic to scientific study, relieved his son-in-law of all financial concerns and enabled him to devote himself exclusively to study and research.

The most notable of Moissan's contributions to science was his isolation (1886) of elementary fluorine and his subsequent extensive study of its properties and behavior. Although the elementary forms of the other halogens had been known for many years (chlorine from 1774, iodine since 1811, and bromine since 1826), and although fluorides had been recognized since 1813, none of the many attempts to obtain the free element itself had been successful. Moissan became interested in this topic in 1884 and after many fruitless approaches was finally successful on June 26, 1886. This was achieved on the demonstration bench of a lecture theatre of a building in the Rue Michelet, Paris, where, since appropriate electric current was available, Charles Friedel (1832-1899) had given Moissan permission to work between lectures. By means of the electrolysis in a platinum apparatus of anhydrous hydrogen fluoride containing dissolved potassium fluoride, the free elementary fluorine was finally obtained. This important result was two days later communicated to the French Academy of Science by Debray and the facts soon confirmed by a committee of that body. Moissan's own report on the isolation of fluorine appeared [Ann. chim. phys. (6) *12,* 472-537 (1887) cf. Nature *37,* 179-182 (1887)] in the following year. Moissan then devoted himself to extensive improvements of his method and apparatus and to protracted studies of the behavior of gaseous fluorine with other elements and compounds [Ann. chim. phys. (6) *24,* 224-288 (1891); cf. Nature *44,* 622-626 (1891)]. Although space forbids any detailed treatment of this program, it resulted eventually in the discovery with Le Beau in 1900 of the now very important sulfur hexafluoride SF_6 [Compt. rend. *130,* 865-871 (1900); Bull. soc. chim. (3) *27,* 230-240 (1902)], thionyl fluoride SOF_2 [Bull. soc. chim. (3) *27,* 240-246 (1902)], and sulfuryl fluoride SO_2F_2 [Bull. soc. chim. (3) *27,* 246-254 (1902)]. Meantime, during a visit to the Royal Institution in London, Moissan and (later Sir) James Dewar (1842-1923) effected on May 29, 1897, the first liquefaction of elementary fluorine, and Dewar first obtained solid fluorine six years later (1903). Although in no way detracting from the immense accomplishments of Moissan in the isolation and study of elementary fluorine, the fact that Bohuslav Brauner (1855-1935) had shown four years before Moissan's success that elementary fluorine was evolved upon heating certain complex cerium fluorides [J. Chem. Soc. *41,* 68-77 (1882); Ber. *14,* 1944-1946 (1882); Monatsh. *3,* 1-60 (1882)] appears to have been little recognized.

Moissan's second great contribution to science is represented by his development of the electric furnace and its utilization in the extensive study of chemical reactions at very high temperatures. He devised (1892) an electric furnace yielding temperatures as high as 3500°C., by whose use he was able to prepare metallic uranium, vanadium, zirconium, tungsten, molybdenum, manganese, chromium, tantalum, titanium, niobium, and thorium. He prepared many new metallic carbides, borides, and silicides. In the course of this work, he obtained [Compt. rend. *116,* 1222-1224; *117,* 425-428 (1893)] silicon carbide, but meantime a much more practical method for obtaining this important abrasive had independently been discovered, patented [U. S. Pat. 492,767 Feb. 28, 1893] and fully described [J. Franklin Inst. *136,* 194-203, 279-289 (1893)] by Edward Goodrich Acheson (1856-1931), who had named it "Carborundum". Moissan's electric furnace experiments also led him to a method for the artificial preparation of small diamonds [for important review of this matter see Trans. Am. Electrochem. Soc. *12,* 39-63 (1907)] but this discovery, although naturally seized upon by the popular fancy and press, was actually but a minor contribution as compared with his others.

In addition to some three hundred journal papers recording his results, Moissan summarized in two substantial books the principal interests of his professional career. His work on fluorine was collected (1887) in "Le Fluor" and that on the electric furnace (1897) in "Le Four Électrique". He also prepared (1904-06) with collaborators a five-volume treatise on inorganic chemistry, the "Traité de chimie Minérale".

Moissan received (1887) the Prix Lacaze of the French Academy of Sciences, to which he was elected (1891) succeeding A. A. T. Cahours (1813-1891). He was awarded (1896) the Davy Medal of the Royal Society of London, which later (1905) named him to foreign membership. His greatest honor, however, was the Nobel Prize for Chemistry (1906) awarded in recognition of his isolation and investigation of elementary fluorine and for his introduction to the service of science of the Moissan electric furnace. It is perhaps worthy of note that apart from Moissan only two other strictly inorganic chemists have received Nobel recognition, viz., Alfred Werner (1866-1919) and Theodore William Richards (1868-1928), who were so honored in 1913 and 1915 respectively.

Moissan's only son, Louis Moissan, was killed (1915) during World War I. Prior to his military service he had been an assistant at the École de Pharmacie, to whose reputation his father had so greatly added, and bequeathed to this institution 200,000 francs for the establishment of two prizes in memory of his parents, one for chemistry (prix Moissan) and one for pharmacy (prix Lugan).

P_1B J. Chem. Soc. *101,* 477-488 (1912) (Moissan Memorial Lecture, also reprinted in Chemical Society Memorial Lectures, Vol. II (1901-1913), pp. 187-198, published by the Chemical Society, London, (1914): B Proc. Roy. Soc. (London) *80-A,* xxx-xxxvii (1907-08): P_3B Chemistry and Industry *1946,* 306-308: P_1B Bull. soc. chim. (4) *3,* No. 1 (?), i-xxxviii (1908) (incl. bibliog.): P_1B Ber. *40,* 5099-5130 (1907): B Angew. Chem. *50,* 859-865 (1937): P_3B Chem. Ztg. *30,* 1219-1220 (1906); *31,* I, 311-314 (1907): B Chem. News *95,* 104-105 (1907): P_1B J. Chem. Education *6,* 1608-1610 (1929); P_3B *ibid., 9,* 1930-1936 (1932): B Chimie et industrie, Spec. No. 113-127 (1932).

NICHOLS, WILLIAM HENRY

Born Jan. 9, 1852, at Brooklyn, N. Y.; died Feb. 21, 1930, while on vacation at Honolulu, Hawaii, age 78.

This American captain of industry studied (1865-68) at Brooklyn Polytechnic Institute and under Dr. John W. Draper (1811-1882) (first president of the American Chemical Society) at New York University, receiving the latter's degrees of S.B. (1870) and M.S. (1873). In later life numerous honorary doctorates were awarded him by other American universities.

Even before completing his work at New York University, he established (1871) at the age of 19 the chemical manufacturing firm of G. H. Nichols and Company, his father's name being used because the son was still legally a minor. By 1890, when the company was producing heavy chemicals and had undertaken the smelting and refining of copper, the firm name was changed to Nichols Chemical Company of New York. Soon the manufacturing aspects of this firm were consolidated with eleven other independent companies to form (1899) the General Chemical Company which under Dr. Nichols as president (1899-1907) and chairman of the board (1907-20) became the largest American producer of heavy chemicals. The metallurgical aspect was made the exclusive concern of the Nichols Chemical Company, which shortly (1905) became the Nichols Copper Company, whose Brooklyn plant developed into one of the world's largest copper refineries.

In 1913 the General Chemical Company joined with the Semet-Solvay Company and the Barrett Manufacturing Company to form the Benzol Products Company, which became (1918) the National Aniline and Chemical Company. On Dec. 30, 1920, Dr. Nichols organized the new Allied Chemical and Dye Corporation, a consolidation of the General Chemical, Solvay Process, Barrett and National Aniline and Chemical Companies, and served (1921-1930) until his death as chairman of the board of this combination, the securities of which were estimated (1929) to be about $750,000,000.

Dr. Nichols was the next to the last surviving member of the 35 chemists who were present at the organization meeting of the American Chemical Society held on April 6, 1876, in the rooms of the New York College of Pharmacy in the New York University Building. As one of the first Americans to recognize the importance of research to industry, he established (1902) a fund for the annual award of a Nichols Medal by the New York Section of the American Chemical Society with the sole restriction that it should be for original work which would stimulate still further research in chemistry. Dr. Nichols served (1904) as president of the (British) Society of Chemical Industry, an honor which, up to his time, had been accorded to only one other American, C. F. Chandler (1899-1900), and which since has come only to four, viz., M. T. Bogert (1912-3), Ira Remsen (1909-10), A. D. Little (1928-9), and W. P. Cohoe (1943-4). He was a delegate to the Seventh International Congress of Applied Chemistry (London, 1909) and president of the Eighth (Washington and New York, 1912).

His many philanthropies include the gift to New York University of its Nichols Chemical Laboratory (dedicated 1927), together with a residuary legacy estimated at $2,000,000.

P_1B J. Am. Chem. Soc. *52* (Proc.) 47-50 (1930): P_3B Ind. Eng. Chem. *15,* 424-425 (1923); *18,* 317-319 (1926); *22,* 394 (1930); *10,* 92 (1918): P_3B J. Soc. Chem. Ind., Special Jubilee No., 72 (July 1931): B J. Ind. Eng. Chem. *13,* 355-357 (1921): P_3B Chem. Markets *26,* 287 (1930): B Science *71,* 528 (1930): P_1B Natl. Cycl. Am. Biog. *24,* 285-286 (1935).

POYNTING, JOHN HENRY

Born Sept. 9, 1852, at Monton, near Manchester, Lancashire, England; died March 30, 1914, at Birmingham, Warwickshire, England, age 62.

This British physicist studied (1867-72) at Owens College, subsequently to become the University of Manchester. Since at that time it had not the authority to grant degrees, Poynting took the examinations of the University of London, receiving (1872) its B.Sc. He then studied (1872-76) at Trinity College of the University of Cambridge, continuing there with great distinction his training in mathematics and physics. His professional career began with two years (1876-78) as demonstrator at Owens College under Prof. Balfour Stewart (1828-1887), during which he began a warm and lifelong friendship with J. J. Thomson (1856-1940), only four years his junior and still a student. Upon election to a fellowship at Trinity College, Cambridge, he spent two further years (1878-80) in beginning his work on the density of the earth. He then (1880) became professor of physics at the newly established Mason College, whose initial faculty of four included also T. W. Bridge (biology), M. J. M. Hill (mathematics), and (later Sir) William Tilden (1842-1926) (chemistry). When Mason College subsequently (1897) became the University of Birmingham, Poynting continued in the chair of physics and thus accomplished most of his life work during 34 years (1880-1914) at the same institution.

Poynting's more important scientific contributions may be mentioned under three headings. The earliest comprised his protracted studies "On a Determination of the Mean Density of the Earth and the Gravitation Constant by Means of the Common Balance" [Phil. Trans. Roy. Soc. (London) *A-182,* 565-656

(1891)], a process popularly known as "weighing the earth". In addition to its specific objectives, this long investigation added much to our knowledge of the techniques of accurate weighing. Late in life Poynting devoted much attention to studies of radiation and the pressure of light. His most important work, however, concerned the flow of electric and magnetic energy and led to the formulation of the so-called Poynting theorem and the Poynting vector. These, comprising one of the most important contributions to electromagnetic theory, were published in two papers entitled "The Transfer of Energy in the Electromagnetic Field" [Phil. Trans. Roy. Soc. (London) *A-175,* Pt. II, 343-361 (1885)] and "Electric Currents and the Electric and Magnetic Induction in the Surrounding Field" [Phil. Trans. Roy. Soc. (London) *A-176,* 277-306 (1886)].

Poynting was elected (1888) to the Royal Society of London, received (1905) its Royal Medal, served (1909-11) on its Council and was a vice president (1910-11). He served (1905-06) as president of the Physical Society of London and was presiding officer of Section A (Physics) of the British Association at its Dover meeting of September 16, 1899, memorable for his friend J. J. Thomson's announcement of his discovery of the separate existence of electrons.

In addition to his numerous research publications (subsequently issued collectively in 1920), Poynting collaborated with J. J. Thomson in the extended Poynting and Thomson "Textbook of Physics", whose several volumes passed through many editions and since the death of both original authors have evolved into the University Textbook of Physics, whose first volume (1947) represented a 14th edition of the original and whose second (1949) comprised a corresponding 10th edition.

P_1B Proc. Roy. Soc. (London) *92,* i-ix (1916): B Nature *93,* 138-140 (1914): B Dict. Natl. Biog. *1912-1921,* 441-442 (1927).

RAMON Y CAJAL, SANTIAGO

Born May 1, 1852, at Petilla de Aragon, a little village which through a strange geographical freak is situated in the province of Zaragoza but belongs to that of Navarra, Spain; died Oct. 17, 1934, at Madrid, Spain, age 82.

This Spanish histologist, medical investigator, author and statesman, although little known to scientists outside his own professional field, has been characterized by some authorities among the greatest biologists of history. His father, himself a doctor's son of peasant stock, passionately desired a medical career for his own child, but attempts to provide an appropriate educational foundation, first at the College of Esculapian Fathers in Jaca and subsequently at the Institute of Huesca, were frustrated by the headstrong resistance of the boy. In desperation, the father apprenticed Santiago first to a barber and afterward to a shoemaker as punishment for his rebellious attitude toward serious study. This extreme measure so far effected the desired discipline, however, that at age 16 the son entered (1868) the University of Zaragoza, in which soon afterward (1870) his father also secured an appointment as professor in the faculty of medicine. Soon after obtaining (1873) his license to practice medicine, the son obtained a commission in the Army Medical Corps and spent a year (1874-75) on service in Cuba. Compelled by ill health to return (1875) to Zaragoza, he joined the University staff as assistant in anatomy, obtained his medical doctorate, and was appointed (1879) Director of the University Museum of Anatomy. His stay of nine years (1875-84) at Zaragoza was followed by brief periods in the chairs of anatomy at Valencia (1884-87) and at Barcelona (1887-92), after which he spent three decades (1892-1922) as professor of histology and pathological anatomy at the University of Madrid. During this tenure he was appointed (1900) director of the newly organized Alfonso XII National Institute of Hygiene, and upon his retirement (1922) the government established a biological research center named in his honor, the Institute Cajal. After a decade of active work in its initial location, this organization was provided (1932) with magnificent new quarters on the hill of San Blas. [For photographs of both old and new buildings, see Sci. Monthly *31*, 180, 181, 182 (1930)].

It is quite beyond the scope of this digest to attempt any detailed survey of Ramon y Cajal's professional contributions. However, it was his work on the fine structure of the nervous system which first established that clear understanding of its functioning that is the basis of modern physiology, psychology, and psychiatry. Early in his professional career he established that the nervous system is composed of conductive chains of individually distinct and discrete nerve cells. Much later the nerve cell was designated by Wilhelm von Waldemeyer (1836-1921) of Berlin as the ''neuron'', and despite the fact that he had at no time contributed either to the experimental discoveries or to the consequent generalizations, the neuron theory is often mistakenly attributed to the German anatomist. In the course of his experimental studies, Ramon y Cajal also did pioneer work in the development of selective procedures for the differential staining of nerve tissue.

Throughout his long life Ramon y Cajal was distressed by the relative isolation of Spanish science and made determined efforts, often at personal expense from meager resources, to improve its means of publication. While still at Barcelona he began (1888) the *Revista trimestral de histología normal y pathólogica* (Trimonthly Review of Normal and Pathological Histology) whose limited edition of sixty copies were distributed to foreign scientists. This was succeeded (1896) by his *Revista trimestral micrográfica* (Trimonthly Micrographical Review). With volume six, this became an annual entitled *Trabajos del laboratoria de investigaciones biológicas* (Works of the Laboratory of Biological Research), published since 1924 in French as the *Travaux du laboratoire de recherches biologique de l'Université de Madrid.*

In addition to these periodicals Ramon y Cajal was the author of several noteworthy scientific books. The earliest of these was his *Manual de histología normal y téchnica micrográfica* (Manual of Normal Histology and Micrographic Technique), comprising 692 pages of small print and 303 woodcuts of his own anatomical preparations, issued in two editions (1889, 1893). His monumental *Textura del sistema nervioso del hombre y de los vertebrados* (Texture of the Nervous System of Man and of the Other Vertebrates), appearing (1899-1904) in two volumes containing 1800 large pages with 897 original illustrations, was revised and amplified in the French edition of 1909-11. This is still regarded as the most complete and accurate description of the more delicate nerve structures ever recorded. This already impressive list of monographs was climaxed (1913-14) by the two large volumes of *Estudios sobre la degeneración y la regeneración del sistema nervioso* (Studies on the Degeneration and the Regeneration of the Nervous System), the publication of which was financed by the physicians of the Argentine Republic and which was issued (1928) in English by the Oxford University Press.

Although his outstanding contributions were but slowly recognized by his contemporaries, honors in abundance were eventually heaped upon Ramon y Cajal. He delivered (1894) the Croonian Lecture of the Royal Society of London. He was one of five European scientists who participated (1899) in the celebration of the first decade of Clark University at Worcester, Massachusetts, U.S.A. He received (1905) the Helmholtz Medal of the Royal Academy of Sciences of Berlin and in the following year (1906) shared with the Italian Camillo Golgi (1843-1926) the sixth annual Nobel Prize to be awarded in the fields of physiology or medicine. He was elected (1909) a foreign member of the Royal Society of London, the first Spanish scientist so honored in more than 150 years, and named (1920) as foreign associate of the U. S. National Academy of Sciences. After his death the Spanish Government undertook a complete republication of his works and proposed the issuance of a commemorative postage stamp showing him at his microscope. His statue is prominent in the Bueno Retiro Garden of Madrid.

In addition to the limited number of selected references cited at the end of this digest, attention is particularly directed to the unusual circumstance of an extensive and fully illustrated autobiography published in the periodical literature. This is his *Recuerdos de mi vida* (Recollections of My Life), translated by E. Horne Craigie and J. Cano from the third Spanish edition (1923) and published in the Memoirs of the American Philosophical Society *8*, Part I, 1-272; Part II, 275-638 (1937). This treatment includes a complete bibliography accompanied by a full list of his distinctions, prizes, honorary degrees and awards. Furthermore, a book biography (303 pages) of Ramon y Cajal under the title "Explorer of the Human Brain" by Dorothy F. Cannon has been published (1949) by Henry Schuman of New York. Both of these easily accessible biographical treatments are warmly commended to the attention of all persons interested in science.

P_1B Biog. Notices Fellows Roy. Soc. (London) *1*, 425-441 (1932-35): P_2B Sci. Monthly *31*, 178-183 (1930); *39*, 567-570 (1934): B Nature *134*, 871-872 (1934): B Naturwissenschaften *23*, 503-506 (1935).

RAMSAY, (SIR) WILLIAM

Born Oct. 2, 1852, at Queen's Crescent, Glasgow, Scotland; died July 23, 1916, at Hazlemere, near High Wycombe, Buckinghamshire, England, age 63.

This truly illustrious chemist, one of the outstanding experimentalists of his (or any other) time, occupies a unique position in chemical history as the discoverer of five elements and the first to recognize transmutation of one element to another. He was the nephew of Sir Andrew C. Ramsay (1814-1891), geologist, and for many years head of the Geological Survey of Great Britain. After a classical education in Glasgow Academy, he entered the University of Glasgow at the age of 14 and during his four year (1866-70) course, which included lectures in physics under William Thomson (1824-1907) (afterward known as Lord Kelvin), became attracted to the pursuit of chemistry. His graduate study began with a single semester (1870-71) at Heidelberg under Robert Wilhelm Bunsen (1811-1899), but Ramsay then transferred to the University of Tübingen where under Rudolf Fittig (1835-1910) he obtained (1872) his doctorate. Returning to his native city, Ramsay served two years (1872-74) in the Young Laboratory of Technical Chemistry of Anderson's College, and afterward six years (1874-80) at the University of Glasgow as assistant to Prof. John Ferguson (1837-1916). At the age of 28 Ramsay was appointed professor of chemistry at University College, Bristol, England, where, during most of his seven-year (1880-87) stay, he also served as principal. In 1887 Ramsay succeeded the renowned Alexander W. Williamson (1824-1904) as professor of chemistry at University College, London, where for approximately a quarter century until his retirement in 1913 he carried on his most important work.

Ramsay's professional contributions may be arranged in four principal groups, the earliest of which was primarily concerned with organic chemistry. His doctoral dissertation [Ann. *168,* 242-253 (1873); J. Chem. Soc. *25,* 491 (1872)] demonstrated the structure of the toluic acid of m.p. 104° as the *ortho* isomer. In collaboration with J. J. Dobbie (1852-1924) at the University of Glasgow, Ramsay showed [J. Chem. Soc. *33,* 102-104 (1878); *35,* 189-196 (1879)] that quinine, quinidine, cinchonine, and cinchonidine upon oxidation with permanganate all gave the same pyridine-2,3,4-tricarboxylic acid (α-carbocinchomeronic acid), incidentally demonstrating for the first time the important relation of these alkaloids to pyridine. During this period he also effected [Phil. Mag. (5) *2,* 271 (1876); (5) *4,* 241 (1877)] the first synthesis of pyridine by passing through a hot tube a mixture of hydrogen cyanide and acetylene.

Upon removing from Glasgow to Bristol, however, Ramsay's research interests shifted to physical chemistry. The numerous publications of this period include the studies with his assistant

Sydney Young (1857-1937) of the relations between vapor pressure and temperature, the evaporation and dissociation of liquids, and the critical state. With Young he devised [J. Chem. Soc. *47,* 640-657 (1885)] the method for maintaining constant temperatures by means of the vapors of liquids of appropriate boiling points which has since become commonplace procedure. He made extensive studies of the molecular complexity of pure liquids, verified [J. Chem. Soc. *63,* 1089-1109 (1893); Phil. Trans. Roy. Soc. (London) *184,* 647-673 (1893)] the Eötvös (1848-1919) law of constancy of rate of change of molecular surface energy with temperature and with John Shields (1869-1920) established the well-known Ramsay-Shields equation.

The third category of Ramsay's many achievements comprises his discovery of the rare gases. In 1893 subsequent to the discovery of Robert John Strutt, the Third Lord Rayleigh (1842-1919), that the density of supposedly pure nitrogen obtained from air was slightly greater than that of samples prepared by chemical means, Ramsay responded to the published [Nature *46,* 512-513 (1892)] appeal of Rayleigh for chemical assistance, and received permission to experiment with atmospheric nitrogen. His observations soon led to direct collaboration with Lord Rayleigh and resulted in the announcement (1894) and publication [Proc. Roy. Soc. (London), *57,* 265-287 (1895)] of the discovery of a new element, a chemically inert gas which was named argon. Ramsay's subsequent search for other sources of this then unique element led him to heat samples of the mineral cleveite and to the discovery (1895) of the first terrestrial occurrence of a second inert gas whose lines had long since (1868) been observed in the solar spectrum and which had already been named helium by Sir J. Norman Lockyer (1836-1920). Although the demonstration of the presence of helium in air was to become (1895) the work of Heinrich J. G. Kayser (1853-1940), Ramsay had by this time been led to suspect the existence of still other inert gases. With the collaboration of Morris W. Travers (1872-) he presently obtained (1898) by careful fractionation of some fifteen liters of supposedly pure argon from liquid air, the still rarer inert elements krypton, neon, and finally xenon. [Many fascinating details of the story of the discovery of the rare gases are recorded elsewhere, notably in Ramsay's own book "The Gases of the Atmosphere; the History of their Discovery", Macmillan and Co., London, 1915; in that of M. W. Travers' "The

Discovery of the Rare Gases", Edward Arnold and Co., London, 1928; and in M. E. Weeks' "Discovery of the Elements", 5th edition, pages 469-481, published 1948 by the Journal of Chemical Education.]

The fourth and undoubtedly most important chapter of Ramsay's contributions to science was that associated with his studies of the decomposition of radium. Following the discovery (1898) of this element through the joint work of Pierre Curie (1859-1906) and his wife Marie Skladowska Curie (1867-1934), Ramsay and his collaborator Frederick Soddy (1877-) showed [Proc. Roy. Soc. (London) *72,* 204-207 (1903)] that the spontaneous decomposition of radium gave *both* helium and another new inert gas [the latter having already (1900) been noted by Fredrich Ernst Dorn (1848-1916)] designated by Ramsay as "niton" but subsequently named radon. This represented the first definite evidence of the transmutation of one element to another and may thus be regarded as the birth of the science of radiochemistry and the study of atomic energy. During further investigations of this field, Ramsay, together with Robert Whytlaw-Gray (1877-) reported [Proc. Roy. Soc. (London) *84,* 536-550 (1910-11)] one of the most remarkable pieces of experimental work ever accomplished on a micro scale up to that time. By direct weighing of a barely perceptible bubble of radium emanation, amounting only to 0.0658 cubic millimeters (i.e. to 6.58×10^{-5} cubic centimeters) they determined its density, incidentally calculating its atomic weight and determining its boiling point, melting point, critical temperatures and critical pressure.

Ramsay was active in the professional societies of his period and deeply concerned with problems of higher education. He was a charter member of the (British) Society of Chemical Industry and later (1903-04) served as its president. He was one of three British delegates to the famous commission which established (1892) the Geneva nomenclature [for group photograph see J. Chem. Soc. *1938,* facing page 1117]. He later served as president (1907-09) of the Chemical Society of London and (1911) of the British Association. He was appointed (1900) adviser to the government of India on the establishment of a new University from funds bequeathed by Mr. J. N. Tata of Bombay. This activity eventually resulted in the establishment (1906) at Bangalore of the Indian Institute of Science, of which Ramsay's assistant Morris W. Travers served (1906-14) as the first director.

In recognition of his accomplishments Ramsay was showered with many honors and eventually was elected to most of the world's scientific academies and societies. He was elected (1888) to the Royal Society of London, was awarded (1895) its Davy Medal and was to have received (November 1916) its highest award, the Copley Medal. Together with Lord Rayleigh he was awarded (1895) the Hodgkins Prize of $10,000 by the Smithsonian Institution of Washington. He received (1897) the Longstaff Medal of the Chemical Society of London, was knighted (1902) by the British Crown, and received the Nobel Prize in Chemistry in the same year (1904) as that in which his earlier colleague Lord Rayleigh was awarded the corresponding Nobel Prize in Physics.

[In addition to the biographical material contained in the scientific periodical literature cited below, important treatments may be found in several books, including Sir William Tilden's ''Life of Sir William Ramsay'', Macmillan and Co., London, 1918; Benjamin Harrow's ''Eminent Chemists of Our Time'', 2nd edition, pp. 41-58, 286-309, D. Van Nostrand Co., Inc., New York, 1927; and by M. W. Travers in ''British Chemists'', pp. 146-175, The Chemical Society of London, 1947.]

P_1B Proc. Roy. Soc. (London) *93-A,* xlii-liv (1917): B J. Chem. Soc. *111,* 369-376 (1917) reprinted from J. Soc. Chem. Ind. *35,* 877-880 (1916): B J. Franklin Inst. *186,* 29-55 (1918) (incl. bibliog.); *182,* 267-270 (1916): B Nature *88,* 339-342 (1912); *97,* 482-485 (1916); *115,* 47, 121-122 (1925); *135,* 619 (1935): B J. Am. Chem. Soc. *39,* Proceedings 19-24 (1917): B Sci. Monthly *9,* 167-178 (1919): B Bull. soc. chim. (4) *25,* 401-426 (1919) (incl. bibliog.): B Chem. Ztg. *40,* 733-735 (1916): B Z. angew. Chem. *22,* 529-530 (1909): B Analyst *41,* 329-333 (1916): B Proc. Am. Phil. Soc. *56,* iii-viii (1917): P_2B J. Chem. Education *4,* 812 (1927): P_3B J. Soc. Chem. Ind., Spec. Jubilee No., 71 (1931): B Dict. Natl. Biog. *1912-21,* 444-446.

STILLMAN, JOHN MAXSON

Born April 14, 1852, in New York City, N. Y.; died Dec. 14, 1923, at Stanford University, Palo Alto, California, age 71.

This American chemist, educator, administrator and historian of chemistry removed to California at the age of 9 and his early education was obtained in the public schools of San Francisco. He entered (1870) the University of California which had been opened the previous year and was then located in Oakland. After graduating (Ph.B. 1874) he declined the post of private secretary to ex-Governor Leland Stanford in order to pursue graduate study in chemistry, first at the University of California and later (1875-76) in Germany at the Universities of Strassburg and Würzburg. Upon his return to America he served for six years (1876-82) as instructor in general and organic chemistry at his alma mater which subsequently awarded him the degrees of Ph.D. (1885) and LL.D. (1916). At this juncture, however, Stillman accepted an offer to enter industry as chief chemist and superintendent of the Boston and American Sugar Refining Companies and during his nine-year stay (1882-91) also lectured on sugar technology at the Massachusetts Institute of Technology, then located in Boston.

Dr. Stillman's principal professional career, however, was devoted to Leland Stanford University, with which in various capacities he was connected for the remaining 31 years (1892-1923) of his life. This institution had been founded (1885) by Leland Stanford (1824-1893) and Mrs. Stanford as a memorial to their son, Leland Stanford, Jr., who had died (1884) at the age of fifteen. Its initial endowment of $20,000,000 was more than doubled after the death of the ex-governor and senator so that by 1901 it was the wealthiest educational institution in the world. Soon after it opened its doors to students in the fall of 1891, Dr. Stillman became head of its department of chemistry, a post which he retained (1892-1917) until his retirement. In fact, of the fifteen professors with which the university began its distinguished career, Dr. Stillman was the only one selected by the founder himself. James M. Stillman's father, Dr. J. D. B. Stillman (1819-1888), had been closely associated with the ex-governor, particularly in connection with the application to the latter's interest in the breeding and racing of horses of the methods of instantaneous photography which eventually resulted (1882) in the famous volume, "The Horse in Motion," executed and published under the auspices of Leland Stanford (Sr.). During the process of assembling a new faculty Mr. Stanford recalled that his associate's son had left his teaching career at the University of California to enter industry and arranged to effect his return to the academic field. His work in the organization and development of the University's Department of Chemistry led (1913) to his selection as vice-president of the institution.

Stillman's great services as teacher, administrator and lovable human being have been recorded by a memorial volume containing essays by five colleagues, complete bibliography, and portrait [Stanford University Press, 1924]. His earlier chemical work was concerned with a variety of topics, but in later life his interest concentrated upon history of chemistry. Of the papers on this theme [reviewed in J. Chem. Education *6,* 466-472 (1929)] his most original is "Petrus Bonus and supposed chemical forgeries" [Sci. Monthly *17,* 318-325 (1923)]. In addition he published two books, one an important research on the life and work of Paracelsus (1920), the other, "The Story of Early Chemistry" (published posthumously 1924), a landmark among English expositions of the history of chemistry.

[The subject of this brief digest is not to be confused with Thomas Bliss Stillman (1852-1915), American chemist and chemical engineer, for whose history see Dict. Am. Biog. *18,* 27-28 (1936) and Natl. Cycl. Am. Biog. *16,* 191 (1918)].

P_1B Isis *34,* 142-146 (1942) (incl. bibliog.): P_1B J. Chem. Education *6,* 466-472 (1929): P_3B Ind. Eng. Chem. *15,* 1283 (1923): B Science *59,* 270 (1924): P_3B Natl. Cycl. Am. Biog. *20,* 145 (1929).

THOMSON, THOMAS

Born April 12, 1773, at Crieff, Perthshire, Scotland; died July 2, 1852, at Kilmun, Argyleshire, Scotland, age 79.

This Scotch chemist, editor, and pioneer historian of chemistry is not to be confused either with Thomas Thomson (1768-1852), the contemporary British jurist and legal antiquary, or with his own son Thomas Thomson (1817-1878), distinguished botanist and explorer. The subject of this digest, after a classical education at Crieff and Stirling, spent three years (1787-90) at the University of St. Andrews, Scotland. He then (1791) removed to Edinburgh where subsequently (1795-99) he studied medicine and obtained his M.D. (1799) with a dissertation "De Aere Atmospherico". For the next decade (1800-11) he remained at the University of Edinburgh as lecturer in chemistry and during this period opened (1807) a laboratory for the instruction of students which was the first of its kind in Great Britain. Following his election (1811) to the Royal Society of London, the next six years were spent in that city mainly in literary pursuits. At the age of 44 and on the recommendation of Sir Joseph Banks (1743-1820), Thomson was appointed (1817) lecturer on chemistry at the University of Glasgow, Scotland, where in the following year (1818) he became its first Regius Professor of Chemistry, a chair which he retained for 34 years (1818-52) until his death and whose next occupant was Thomas Anderson (1819-1874). Owing to a decline of Thomson's health, his nephew Robert Dundas Thomson (1810-1864) assisted his uncle for four years and carried on all his uncle's duties for the last six years (1846-52) of his life.

At the age of 23 Thomson succeeded (1796) his elder brother James as Editor of the Supplement to the Encyclopedia Britannica and the articles which he also contributed to it formed the basis for his later famous "System of Chemistry" (1st ed. 1802, 4 volumes . . . 6th ed. 1820, 4 volumes, also several French, German, and American editions). The third edition (1807) was especially notable in containing the first detailed account of the atomic theory of John Dalton (1766-1844) which had been communicated to him during a visit to Dalton in Manchester in August 1804 and was both fuller than and substantially antedated the account published in 1808 by Dalton himself. The only previous comparable work was the great treatise (1801, 11 vols.) of Antoine-François de Fourcroy (1755-1809) which was, however, inferior to Thomson's in both content and style as well as too closely confined to French discoveries. The "System" was eventually split into separate works on inorganic chemistry (1831), organic chemistry (1838, 1843), mineralogy (1836), and heat and electricity (1830, 2nd ed. 1840). After Thomson's death the "System of Chemistry" was superseded by Leopold Gmelin's (1788-1853) "Handbuch der anorganischen Chemie", first published in two volumes (1817-19) and subsequently greatly expanded. Other important books by Thomson include his (1812) "History of the Royal Society (of London)" and his (1830-31) two-volume "History of Chemistry", the latter long comprising the only such book in English.

During his residence in London, Thomson founded (1813) the monthly *Annals of Philosophy* (vols. 1-16, 1813-20, continued by Richard Phillips (1778-1851) vols. 17-28, 1821-26; then merged into the *Philosophical Magazine*) [for history of latter see its special separate Commemoration Number *1948,* pp 1-9]. In addition to original articles this journal introduced many novel features including abstracts and critical annotations of European research, and the first annual reports on the progress of science, particularly chemistry. After the publication of his "History of Chemistry", Thomson concentrated his writing on biographical accounts of important European and British chemists, most of which were published in the *Annals of Philosophy*.

In addition to his extensive writing and editing, Thomson carried out a large amount of experimental work and his published papers comprise 201 titles. He discovered (1803) sulfur (mono) chloride and gave (1808) the first example of the Law of Multiple Proportions by showing that in the normal and acid oxalates of potassium and strontium, one salt contains (for the same amount of acid) just double the proportion of base contained in the second. He was the first to describe (1804) the determination of the density of a solid by flotation in a liquid [cf. J. Chem. Education *25,* 307 (1948)], thus anticipating the use of this method by Davy [Phil. Trans. *98,* 21 (1808)] or by DuFour [Compt. rend. *50,* 1039 (1860), *54,* 1079 (1862)]. He introduced [Nicholson's J. *8,* 280 (1804)] the names protoxide, deutoxide, tritoxide, and peroxide. The claim that Thomson was first to introduce chemical symbols is unfounded but he may have been first (1808) to use chemical symbols in a quantitative sense [cf. J. Soc. Chem. Ind. *55,* 759-762 (1936)].

B Ann. Sci. *6,* 115-126 (1949): P_1B Chymia *1,* 37-53 (1948) (incl. bibliography of publications on history of chemistry): P_3 J. Chem. Education *9,* 871 (1932): B Dict. Natl. Biog. *56,* (1898).

WHEATSTONE, (SIR) CHARLES

Born Feb. 6,* 1802, at Gloucester, Gloucestershire, England; died Oct. 19, 1875, at Paris, France, age 73.

This British inventor and physicist, an uncle of Oliver Heaviside (1850-1925), and one of the most versatile and productive minds of the 19th century, left behind him a long record of valuable practical and scientific accomplishment in the fields of sound, light, and electricity. Little is known of his early life. He was the son of a music seller and together with a brother established in London (1823) at the age of 21 a business devoted to the making of musical instruments. Although he appears never to have had any formal college training, his scientific and technical accomplishments during the next decade attracted such favorable attention that he was appointed (1834) professor of natural philosophy at King's College, London. Owing, however, to an almost morbid timidity in the presence of an audience, an idiosyncrasy which persisted throughout his life, Wheatstone's connection with this post was largely nominal, and many of his inventions were made public through the mouth of his friend Michael Faraday (1791-1867). Nevertheless, Wheatstone ultimately bequeathed to the college his collection of books and instruments, now preserved in its Wheatstone Laboratory. Despite his lack of formal scientific preparation or college degree, his work was recognized by his election (1836) to the Royal Society of London, from which he afterward also was twice (1840, 1843) awarded a Royal Medal and later (1868) the Copley Medal, its highest honor. He was knighted in the same year (1868) and became soon after (1873) a Foreign Associate of the French Academy of Sciences. At the time of his death he held 34 distinctions from governments, universities, or learned societies in 13 countries, including honorary degrees from both Cambridge and Oxford.

---

* The day of Wheatstone's birth, not previously reported in the literature, was kindly supplied for this article by Dr. D. C. Martin from records of the Royal Society of London.

Wheatstone's earliest achievements were in the field of acoustics. He first attracted attention (1821) at the age of 19 with his "Enchanted Lyre", essentially a simple device for the emission of music transmitted to it through a steel rod from its point of origin some distance away. Two years later (1823) he published his first scientific paper, dealing with certain of his acoustic experiments, in the new journal [Annals of Philosophy, (New Series) *6,* 81-90 (1823)] established (1813) by Thomas Thomson (1773-1852). He invented (1827) a device for rendering sound vibrations visible which he designated as a "Kaleidophone", and in the same year was first to employ the word "microphone" in connection with a device for the amplification of feeble sounds. He invented and patented (1829) the musical instrument known as the concertina in the same year as that in which the accordion was invented on the Continent. His principal contribution to scientific acoustics, however, is a memoir [Phil. Trans. Roy. Soc. (London) *1833,* 593-634] in which he first accounted for the formation of the beautiful and varied patterns produced in thin layers of sand by the vibration of a violin bow against the edge of the supporting surface, which having initially been observed by Ernst Florens

Friedrich Chladni (1756-1827) are still known as Chladni's figures.

Wheatstone also made memorable contributions to optics. He was first (1833) to analyze binocular vision and to construct [Phil. Trans. Roy. Soc. (London) *1838,* 371-394; Poggendorf's Ann. Physik *51,* (Suppl.), 1-48 (1839); Ann. chim. phys. (2) *2,* 330-370 (1841)] the now well-known instrument which he named stereoscope. The concept of solidity as derived from mental superposition of two pictures of the same object in dissimilar perspectives is solely due to Wheatstone and it was for his studies of binocular vision that he received (1840) his first Royal Medal. While Wheatstone's instrument was of the reflecting type, (Sir) David Brewster (1781-1868) subsequently effected further developments by employing in his refracting or lenticular stereoscope wedge-shaped segments of large lenses in which the lens and prism arrangement of Wheatstone were combined. [The relevant papers of Wheatstone, Brewster, and others on this topic have been brought together in No. 168 (1908) of the series by Wilhelm Ostwald (1853-1932) known as "*Klassiker der Exakten Wissenschaften*".] Wheatstone was also first (1835) to discover [Brit. Assoc. Rept. for *1835,* Part 2, 11-12; Poggendorf's Ann. Physik *36,* 148-149 (1835)] that the spectrum emitted by the incandescent vapor of metals volatilized by an electric discharge could be resolved into a series of bright lines characteristic of the metal, and to recognize the outstanding importance of this method of spectroscopic analysis subsequently further developed by Robert Wilhelm Bunsen (1811-1899), Gustav Robert Kirchhoff (1824-1887) and others. Wheatstone further devised [Brit. Assoc. Rept. for *1848,* Part 2, 10-12] a "polar clock" capable of determining true solar time, even when the sun itself was obscured, by utilizing (Sir) David Brewster's discovery that the plane of polarization of light from the sky is always 90° from the sun.

Despite Wheatstone's notable contributions to our knowledge of sound and light, some examples of which have just been mentioned, his greatest accomplishments were in the field of electricity. He was first (1834) to measure the speed with which electricity travels through a wire, obtaining a value of 288,000 miles per second. For his experiments [Phil. Trans. Roy. Soc. (London) *1834,* 583-591; Poggendorf's Ann. Physik *34,* 464-480 (1835)] to this end he devised a revolving mirror technique afterwards advantageously employed by Jean Bernard Léon Foucault (1819-1868), Armand Hippolyte Louis

Fizeau (1819-1896), and others, in the measurement of the velocity of light. Although Wheatstone did not invent the electric telegraph and indeed a score of precursors [for tabulation see Telegraphic Journal citation at end] were even then known, he together with his partner Dr. (later Sir) William Fothergill Cooke (1806-1879) were the first to develop it to a practical means of communication, and this fact was recognized in their (1837) British patent. This development went through many stages and controversies which cannot here be detailed, but included the alphabetical dial telegraph (1840), a type printing instrument (1841), the five needle, two needle and finally the single needle instrument (1845) which made its adoption practical. [For many further details see also Nature *11,* 390-392, 450-452, 470-472, 510-512 (1875); *12,* 30-32, 69-72, 110-113, 149-151, 254-256, (1875); J. Roy. Inst. Gt. Brit. *9,* 297-304 (1880).] He devised (1837) a magneto-electric generator employed with his telegraph, and (1840) a method for electrical synchronization of a series of clocks from a standard monitor. He devised and named [Proc. Roy. Soc. (London) *4,* 469-471 (1843)] the rheostat. His name is perhaps most firmly associated with that fundamental instrument for the measurement of electrical resistance which has become known as the "Wheatstone bridge". As Wheatstone himself carefully pointed out, this was initially described in the Bakerian Lecture [Phil. Trans. Roy. Soc. (London) *123,* 95-142 (1833)] of Samuel Hunter Christie (1784-1865), but its utility was popularized by Wheatstone, with whose name it has subsequently been connected. Protracted experiments on the generation of electricity culminated (1867) in the development of a dynamo in which permanent magnets were first replaced by self-exciting electromagnets. Essentially the same discovery had independently been made by Werner Siemens (1816-1892) and both papers were by coincidence read to the Royal Society on February 4, 1867, at the same meeting.

The achievements touched upon in the preceding paragraphs represent only a few of Wheatstone's contributions to the advancement of knowledge. Other interesting items include the invention (1840) of a "chronoscope" for measuring and recording very small intervals of time such as required in studying the velocity of projectiles; an electromagnetic counter for automatically recording any given repeated mechanical action; a device for automatic periodic recording of barometric pressure; a cryptographic machine for ciphering and decoding messages;

the first effective magnetoelectric machine for detonating explosive charges at a distance even under water; and many others.

More extensive accounts of Wheatstone's career may be found in R. Appleyard's "Pioneers of Electrical Communication", Macmillan and Co., London (1930) (which also includes many photographs of his apparatus); in J. Munro's "Heroes of the Telegraph", The Religious Tract Society, London (1891); and W. T. Jeans' "Lives of the Electricians", Whittaker and Co., London (1887). All of Wheatstone's papers were posthumously brought together (1879) in a Collective Volume published by the Physical Society of London.

B Proc. Roy. Soc. (London) *24*, xvi-xxvii (1876): P_1B Nature *13*, 501-502 (1876): P_1B Pop. Sci. Monthly *8*, 363-365 (1876): B Telegraphic Journal [contd. after 1891 as Electrical Review (London)] *3*, 253-257 (1875): B J. Soc. Telegraph Engineers *4*, 319-334 (1875): B Dict. Natl. Biog. *60*, 435-437 (1899).

WIEDEMANN, EILHARD ERNST GUSTAV

Born Aug. 1, 1852, in Berlin, Germany; died Jan. 7, 1928, at Erlangen, Germany, age 76.

This German physicist was the son of the physicist Gustav Heinrich Wiedemann (1826-1899). His mother, independently notable for the publication of several books and translations, was the daughter of the distinguished chemist Eilhard Mitscherlich (1794-1863), who died when his grandson was eleven years old. After the usual preliminary studies in the gymnasia of Basel, Braunschweig, and Karlsruhe during his father' residence at the corresponding universities, the son pursued advanced studies at the University of Heidelberg during which he sat at the feet of Robert Wilhelm Bunsen (1811-1899), Gustav Robert Kirchhoff (1824-1887), and Hermann Ludwig von Helmholtz (1821-1894). His doctoral dissertation on "The Elliptical Polarization of Light and its Relation to Surface Colors", though initiated at Heidelberg, was presented (1872) at the University of Leipzig to which his father meantime had been called. Eilhard Wiedemann's own professional career continued for 14 years at Leipzig, first as docent, later (1878-85) as extraordinary professor. He then accepted (1886) an ordinary professorship at Darmstadt but in the fall of the same year transferred to the University of Erlangen where he remained for forty years (1886-1926) until his retirement.

In order, during the earlier stages of his own professional work, to distinguish himself independently of his famous father, Eilhard Wiedemann deliberately cultivated a technical area distinct from that of his parent. His first substantial research involved an extensive study of the specific heats of gases during which he introduced many refinements representing advances over the prior contributions of Henri Victor Regnault (1810-1878). Later he studied electric discharge through gases and found independently of, but simultaneously with, Johann Wilhelm Hittorf (1824-1919) that light was given off far below 100° C., thus demonstrating that light emission was not merely the result of heating to incandescence as had formerly been supposed. In still later work at Erlangen, Wiedemann discovered (1887) that under certain circumstances the fluorescence of dyestuffs could be extended to phosphorescence, and subsequently developed a terminology for luminescence phenomena which has achieved international acceptance.

In addition to these and other contributions to experimental physics, Eilhard Wiedemann is notable for scientific literary enterprises of two entirely different types. He founded (1877) and for 24 years (1877-1901) edited the important abstract and review journal of physics known as Wiedemann's *Beiblätter*. This publication, undertaken at the suggestion of Johann Christian Poggendorf (1796-1877), comprised a valuable adjunct to the latter's *Annalen der Physik* by directing particular attention to the work of physicists of other countries. His other literary activity was concerned with research in the history of Arabian science, an interest possibly founded on and certainly facilitated by his extraordinary linguistic powers, and regarding which he published more than 250 contributions. (For a complete bibliography of these papers, see the Isis reference below.)

P_1B Isis *14,* 166-186 (1930): B Physik Z. *29,* 185-190 (1928).

For the convenience of users, the names of the 103 persons whose biographical digests have appeared in this and the three preceding annual series are here tabulated with reference to the year of the particular article of this series in which their anniversaries were cited.

Anschütz, R.	1952	Guericke, O. von	1952
Balard, A. J.	1952	Haller, A.	1949
Becquerel, A. H.	1952	Heaviside, O.	1950
Berliner, E.	1951	Hell, C. M. von	1949
Bernouilli, D.	1950	Hempel, W. M.	1951
Billeter, O.	1951	Henniger, A. R. M.	1950
Black, J.	1949	Hepp, E.	1951
Boussingault, J. B.	1952	Herreshoff, J. B. F.	1950
Braun, K. F.	1950	Hess, G. H.	1950, 1952
Brϕgger, W. C.	1951	Hill, H. B.	1949
Brühl, J. W.	1950	Hoff, J. H. van't	1952
Claisen, L.	1951	Hofmeister, F.	1950
Clapeyron, B. P. E.	1949	Hummel, J. J.	1950
Dana, E. S.	1949	Jacobi, K. G. J.	1951
Daguerre, L. J. M.	1951	Jacobi, M. H.	1951
De La Rive, A. A.	1951	Jenner, E.	1949
Del Rio, A. M.	1949	Jones, W.	1949
Demarcay, E. A.	1952	Kingzett, C. T.	1952
Descartes, R.	1950	Kjeldahl, J. G. C. T.	1949
Dixon, H. B.	1952	Klein, C. F.	1949
Dobbie, Sir J. J.	1952	Koenigs, W.	1951
Döbereiner, J. W.	1949	Laplace, P. S.	1949
Doebner, O. G.	1950	Lassaigne, J. L.	1950
Dumas, J. B. A.	1950	LeChatelier, H. L.	1950
Étard, A. L.	1952	Legendre, A. M.	1952
Eykman, J. F.	1951	Lodge, Sir O. J.	1951
Fischer, E.	1952	Maberry, C. F.	1950
Fischer, O. P.	1952	Magnus, H. G.	1952
Fleming, Sir J. A.	1949	McMurtrie, W.	1951
Frasch, H.	1951	Meldola, R.	1949
Gabriel, S.	1951	Michelson, A. A.	1952
Gadolin, J.	1952	Miller, W. H.	1951
Gay-Lussac, J. L.	1950	Moissan, H.	1952
Goldschmiedt, G.	1950	Munroe, C. E.	1949
Goldstein, E.	1950	Napier, J.	1950
Gooch, F. A.	1952	Nichols, W. H.	1952
Goodyear, C.	1950	Noelting, D. E.	1951

Oersted, H. C.	1951
Osler, Sir W.	1949
Pavlov, I. P.	1949
Pechmann, H. von	1950
Plücker, J.	1951
Poynting, J. H.	1952
Ramon y Cajal, S.	1952
Ramsay, Sir W.	1952
Reich, F.	1949
Reverdin, F.	1949
Richet, C. R.	1950
Rutherford, D.	1949
Saussure, H. B. de	1949
Schönbein, C. F.	1949
Schultz, G.	1951
Schuster, Sir A.	1951
Skraup, Z. H.	1950
Stillman, J. M.	1952
Talbot, W. H. F.	1950
Thompson, S. P.	1951
Thomson, T.	1952
Vaughan, V. C.	1951
Wagner, G.	1949
Welch, W. H.	1950
Weston, E.	1950
Weyl, T.	1951
Wheatstone, Sir C.	1952
Wiedemann, E.	1952
Wöhler, F.	1950

Proceedings of the American Academy of Arts and Sciences

Vol. 81, No. 3, P. 101–164 — January, 1952

Winter Number

Officers and Committees for 1951–1952 102

List of the Fellows and Foreign Honorary Members by Class and Section 105

Alphabetical Index to Lists of Members 134

Statutes 147

Charter of Incorporation 162

American Academy of Arts and Sciences
OFFICERS AND COMMITTEES FOR 1951-1952

President
Edwin H. Land

Vice-Presidents

Class I	*Class II*
Jerome C. Hunsaker	Alfred S. Romer
Class III	*Class IV*
Erwin D. Canham	Wallace Goodrich

Secretary	**Treasurer**
Henry B. Phillips	Horace S. Ford
Librarian	**Editor**
Ernest H. Huntress	Taylor Starck

Executive Officer
Ralph W. Burhoe

Councillors

Class I		Class II
Isadore Amdur	*Terms expire 1952*	Arlie V. Bock
Edwin H. Land	*Terms expire 1953*	Francis O. Schmitt
Fred L. Whipple	*Terms expire 1954*	Martin J. Buerger
Harold L. Hazen	*Terms expire 1955*	Kenneth V. Thimann

Class III		Class IV
Keyes DeW. Metcalf	*Terms expire 1952*	Henry R. Viets
W. Rupert Maclaurin	*Terms expire 1953*	George W. Sherburn
Mark DeW. Howe	*Terms expire 1954*	Willard V. O. Quine
Carl J. Friedrich	*Terms expire 1955*	Joseph Hudnut

STANDING COMMITTEES

The House Committee
Chester M. Alter (-1952), *Chairman*
Frank M. Carpenter (-1954)
John B. Wilbur (-1953)

The Committee on Membership
Edwin H. Land, *Chairman, ex officio*

Class I		Class II
Otto Oldenberg	*Terms expire 1952*	Roy G. Hoskins
William H. McAdams	*Terms expire 1953*	Alden B. Dawson

Class III		Class IV
Saville R. Davis	*Terms expire 1952*	Clyde K. M. Kluckhohn
Zechariah Chafee, Jr.	*Terms expire 1953*	Bartlett H. Hayes

The Committee on Meetings
Edwin H. Land, *Chairman, ex officio*
Henry B. Phillips, *Secretary, ex officio*

Class I	Class II
Avery A. Morton (-1952)	Frederick L. Hisaw (-1952)
Jerome C. Hunsaker, *ex officio*	Alfred S. Romer, *ex officio*

Class III

Mark DeW. Howe (-1953)
Erwin D. Canham, *ex officio*

Class IV

Henry J. Cadbury (-1953)
Wallace Goodrich, *ex officio*

The Committee on Finance

Horace S. Ford, *Chairman, ex officio*

Ralph E. Freeman (-1953)
Francis C. Gray (-1953)
Henry P. Kendall (-1952)
Henry L. Shattuck (-1952)

The Auditing Committee

Thomas H. Sanders (-1953)
Donald S. Tucker (-1952)

The Committee on Publication

Taylor Starck, *Chairman, ex officio*

Class I

Ernest H. Huntress (-1952)

Class II

Marland P. Billings (-1952)

Class III

John E. Burchard (-1953)

Class IV

Wilbur K. Jordan (-1953)

The Permanent Science Fund Committee

Hudson Hoagland (-1953), *Chairman*

Helen M. Cam (-1954)
Gustavus J. Esselen (-1953)
A. Baird Hastings (-1952)
Chester S. Keefer (-1952)
Donald H. Menzel (-1954)
Julius A. Stratton (-1952)

The Rumford Committee

Harlow Shapley (-1952), *Chairman*

Percy W. Bridgman (-1952)
Daniel F. Comstock (-1954)
Arthur C. Hardy (-1954)
George R. Harrison (-1953)
Joseph H. Keenan (-1954)
Francis O. Schmitt (-1953)

The C. M. Warren Committee

Frederick G. Keyes (-1952), *Chairman*

Edwin J. Cohn (-1954)
Louis F. Fieser (-1954)
Charles A. Kraus (-1952)
Eugene M. Landis (-1954)
Avery A. Morton (-1953)
Walter C. Schumb (-1953)

The Amory Prize Committee

(1951-52)

Edwin B. Wilson, *Chairman*

William B. Castle
Roy G. Hoskins
Gregory Pincus
George W. Thorn
George B. Wislocki
S. Burt Wolbach

The Nominating Committee

Class I

Charles D. Coryell (-1954)
Julius A. Stratton (-1953)
Edwin B. Wilson (-1952), *Chairman*

Class II

Hudson Hoagland (-1954)
Paul C. Mangelsdorf (-1953)
George W. Thorn (-1952)

Class III

Douglass V. Brown (-1953)
Zechariah Chafee, Jr. (-1954)
Keyes DeW. Metcalf (-1952)

Class IV

C. J. Ducasse (-1952)
Wilbur K. Jordan (-1954)
Edward A. Weeks, Jr. (-1953)

SPECIAL COMMITTEES ACTIVE DURING 1951-1952

Committee on Classification of Membership

K. DeW. Metcalf, *Chairman*

Hudson Hoagland
W. V. O. Quine
Harlow Shapley

Committee for the Conference on Climatic Variations

Harlow Shapley, *Chairman*

Elso S. Barghoorn
Columbus O'D. Iselin
Helmut Landsberg
Hurd C. Willett

Committee on International Relations

John T. Edsall, *Chairman*

Mark DeW. Howe
Paul M. Doty
Taylor Starck
Henry R. Viets

Committee on Planning and Development

Edwin H. Land, *Chairman*

Mason Hammond
Milton E. Lord
Ralph Lowell
George C. Shattuck

Committee on School Science

John H. Welsh, *Chairman*

Chester M. Alter
Isadore Amdur
Francis Bitter
Fletcher G. Watson
Wendell Furry
Albert E. Navez
John G. Read

Committee on Science and Values

Laszlo Tisza, *Chairman*

George Wald
Philipp Frank
Henry B. Phillips
Robert Ulich

Committee on Solar Energy

Harlow Shapley, *Chairman*

Hoyt C. Hottel
Paul C. Mangelsdorf
Donald H. Menzel

Committee on Statutory Amendments

Chester M. Alter, *Chairman*

Horace S. Ford
Harold L. Hazen

Committee on Unity of Science

Percy W. Bridgman, *Chairman*

Horace S. Ford
Philipp Frank

LIST

OF THE

FELLOWS AND FOREIGN HONORARY MEMBERS

(Corrected to January 1, 1952)

FELLOWS—929

(Number limited to one thousand)

The year of election is indicated in the left margin, the century being omitted; thus 21 means 1921, 92 means 1892. When a Fellow resigned and was re-elected, the year of re-election is indicated in the ordinary way, the year of election is enclosed in square brackets. The year of election of an Associate, now a Fellow, is indicated by round brackets.

CLASS I—Mathematical and Physical Sciences—253

Section 1—*Mathematics and Astronomy*—49

21	Charles Greeley Abbot	Washington, D. C.
32	Clarence Raymond Adams	Providence, R. I.
47	Howard Hathaway Aiken	Cambridge
17	Raymond Clare Archibald	Providence, R. I.
46	James Gilbert Baker	Cambridge
46	John Landes Barnes	Fullerton, Cal.
32	Albert Arnold Bennett	Providence, R. I.
51	Stefan Bergman	Cambridge
45	Garrett Birkhoff	Cambridge
39	Bart Jan Bok	Lexington
50	Ira Sprague Bowen	Pasadena, Cal.
51	Dirk Brouwer	New Haven, Conn.
46	Subrahmanyan Chandrasekhar	Williams Bay, Wis.
13	Julian Lowell Coolidge	Cambridge
15	Leonard Eugene Dickson	Joliet, Ill.
33	Jesse Douglas	Brooklyn, N. Y.
38	John Charles Duncan	Tucson, Ariz.
34	Griffith Conrad Evans	Berkeley, Cal.
30	Philip Franklin	Belmont
32	Einar Hille	New Haven, Conn.
13	Edward Vermilye Huntington	Cambridge
49	Witold Hurewicz	Cambridge
38	E[lvin] Morton Jellinek	Fort Worth, Tex.
49	Gerard Peter Kuiper	Williams Bay, Wis.
45	Norman Levinson	Belmont
51	Chia-Chiao Lin	Cambridge

25	Willem Jacob Luyten	Minneapolis, Minn.
44	Saunders MacLane	Chicago, Ill.
48	William Ted Martin	Princeton, N. J.
34	Donald Howard Menzel	Cambridge
23	Samuel Alfred Mitchell	University, Va.
29	Marston Morse	Princeton, N. J.
19	Forest Ray Moulton	Washington, D. C.
44	John von Neumann	Princeton, N. J.
43	Cecilia Payne-Gaposchkin	Lexington
18	Henry Bayard Phillips	Lincoln
50	Eric Reissner	Cambridge
02	Henry Norris Russell	Princeton, N. J.
20	Harlow Shapley	Cambridge
09	Vesto Melvin Slipher	Flagstaff, Ariz.
21	Joel Stebbins	Mt. Hamilton, Cal.
27	*Harlan True Stetson*	*Fort Lauderdale, Fla.*
30	Dirk Jan Struik	Belmont
42	Otto Struve	Williams Bay, Wis.
23	Oswald Veblen	Princeton, N. J.
29	Joseph Leonard Walsh	Cambridge
41	Fred Lawrence Whipple	Cambridge
32	David Vernon Widder	Arlington
48	Oscar Zariski	Cambridge

CLASS I, SECTION 2—*Physics*—74

51	William Phelps Allis	Cambridge
28	Adelbert Ames, Jr.	Hanover, N. H.
50	Carl David Anderson	Pasadena, Cal.
37	Kenneth Tompkins Bainbridge	Cambridge
21	Samuel Jackson Barnett	Pasadena, Cal.
49	Jesse Wakefield Beams	Charlottesville, Va.
42	Francis Birch	Cambridge
50	Francis Bitter	Cambridge
51	Gregory Breit	New Haven, Conn.
12	Percy Williams Bridgman	Cambridge
51	Harvey Brooks	Cambridge
26	Walter Guyton Cady	Middletown, Conn.
03	George Ashley Campbell	Upper Montclair, N. J.
21	Leslie Lyle Campbell	Lexington, Va.
16	Emory Leon Chaffee	Belmont
28	Arthur Holly Compton	St. Louis, Mo.
31	Karl Taylor Compton	Cambridge
12	Daniel Frost Comstock	Lincoln
47	Edward Uhler Condon	Corning, N. Y.
15	William David Coolidge	Schenectady, N. Y.
34	Franzo Hazlett Crawford	Williamstown

13	Henry Crew	Evanston, Ill.
11	Harvey Nathaniel Davis	Montclair, N. J.
29	Clinton Joseph Davisson	Charlottesville, Va.
12	Arthur Louis Day	Bethesda, Md.
45	Robley Dunglison Evans	Belmont
09	Arthur Woolsey Ewell	Worcester
39	Harry Edward Farnsworth	Providence, R. I.
49	James Brown Fisk	Basking Ridge, N. J.
49	Nathaniel Herman Frank	Cambridge
43	Philipp Frank	Cambridge
49	Wendell Hinkle Furry	Cambridge
29	Arthur Cobb Hardy	Wellesley
31	George Russell Harrison	Belmont
14	John Charles Hubbard	Washington, D. C.
17	Gordon Ferrie Hull	Hanover, N. H.
40	Frederick Vinton Hunt	Belmont
22	Edwin Crawford Kemble	Cambridge
44	Ronold Wyeth Percival King	Winchester
43	Edwin Herbert Land	Cambridge
37	Ernest Orlando Lawrence	Berkeley, Cal.
51	Philippe Emmanuel Le Corbeiller	Cambridge
31	Robert Bruce Lindsay	Providence, R. I.
01	Theodore Lyman	Brookline
34	Louis Williams McKeehan	New Haven, Conn.
14	Robert Andrews Millikan	Pasadena, Cal.
34	Harry Rowe Mimno	Lexington
34	Philip McCord Morse	Cambridge
49	Hans Mueller	Cambridge
31	Otto Oldenberg	Cambridge
40	J. Robert Oppenheimer	Princeton, N. J.
34	Leigh Page	New Haven, Conn.
07	George Washington Pierce	Cambridge
50	Edward Mills Purcell	Cambridge
50	Norman Foster Ramsey	Cambridge
48	Bruno Benedetto Rossi	Winchester
48	Julian Seymour Schwinger	Boston
27	John Clarke Slater	Cambridge
37	George Walter Stewart	Iowa City, Ia.
43	Donald Charles Stockbarger	Belmont
46	Julius Adams Stratton	Belmont
37	Jabez Curry Street	Belmont
50	Laszlo Tisza	Cambridge
50	Merle Antony Tuve	Washington, D. C.
28	Manuel Sandoval Vallarta	Mexico, D. F.
35	Robert Jemison Van de Graaff	Belmont
34	John Hasbrouck Van Vleck	Cambridge

35	Bertram Eugene Warren	Arlington
18	David Locke Webster	Palo Alto, Cal.
48	Victor Frederick Weisskopf	Cambridge
50	Eugene Paul Wigner	Princeton, N. J.
11	Edwin Bidwell Wilson	Brookline
13	Robert Williams Wood	Baltimore, Md.
49	Jerrold Reinach Zacharias	Cambridge

Class I, Section 3—*Chemistry*—64

26	Roger Adams	Urbana, Ill.
44	Isadore Amdur	Cambridge
45	Eric Glendinning Ball	Newton Highlands
13	Wilder Dwight Bancroft	Ithaca, N. Y.
46	Paul Doughty Bartlett	Weston
07	Gregory Paul Baxter	Cambridge
29	James Alexander Beattie	Belmont
14	Marston Taylor Bogert	New York, N. Y.
49	William Clouser Boyd	Boston
38	Emile Monnin Chamot	Ithaca, N. Y.
51	Robert Hugh Cole	Providence, R. I.
42	Samuel Cornette Collins	Belmont
24	James Bryant Conant	Cambridge
45	Arthur Clay Cope	Belmont
48	Carl Ferdinand Cori	St. Louis, Mo.
48	Charles DuBois Coryell	Lexington
47	Paul Clifford Cross	Seattle, Wash.
51	Paul Mead Doty	Cambridge
37	John Tileston Edsall	Dedham
37	Gustavus John Esselen	Swampscott
33	Louis Frederick Fieser	Belmont
50	William Francis Giauque	Berkeley, Cal.
35	Louis Harris	Belmont
36	Albert Baird Hastings	Brookline
38	Robert Casad Hockett	Concord
36	Ernest Hamlin Huntress	Melrose Highlands
19	Frederick George Keyes	Cambridge
49	John Gamble Kirkwood	New Haven, Conn.
33	George Bogdan Kistiakowsky	Cambridge
15	Charles August Kraus	Providence, R. I.
14	Arthur Becket Lamb	Brookline
18	Irving Langmuir	Schenectady, N. Y.
15	Warren Kendall Lewis	Newton
49	James Joseph Lingane	Cambridge
49	Fritz Albert Lipmann	Boston
50	Richard Collins Lord	Cambridge

23	Duncan Arthur MacInnes	New York, N. Y.
41	Charles Edward Kenneth Mees	Rochester, N. Y.
35	Nicholas Athanasius Milas	Belmont
36	Avery Adrian Morton	Watertown
19	Edward Mueller	Cambridge
49	John Howard Northrop	Berkeley, Cal.
31	William Albert Noyes, Jr.	Rochester, N. Y.
45	John Lawrence Oncley	Newtonville
49	Lars Onsager	New Haven, Conn.
44	Linus Carl Pauling	Pasadena, Cal.
49	Eugene Rochow	Cambridge
28	George Scatchard	Cambridge
32	Walter Cecil Schumb	East Milton
51	John Clark Sheehan	Cambridge
15	Miles Standish Sherrill	Brookline
34	Leighton Bruerton Smith	Beverly
49	Wendell Meredith Stanley	Berkeley, Cal.
47	Clark Conkling Stephenson	Cambridge
46	Walter Hugo Stockmayer	Weston
49	James Batcheller Sumner	Ithaca, N. Y.
43	Hugh Stott Taylor	Princeton, N. J.
38	Harold Clayton Urey	Chicago, Ill.
48	Vincent du Vigneaud	New York, N. Y.
11	Willis Rodney Whitney	Schenectady, N. Y.
44	Edgar Bright Wilson, Jr.	Cambridge
51	Melville Lawrence Wolfrom	Columbus, Ohio
48	Robert Burns Woodward	Cambridge
41	Ralph Chillingworth Young	Arlington

CLASS I, SECTION 4—*Technology and Engineering*—66

06	Comfort Avery Adams	Philadelphia, Pa.
33	Harold Kilbrith Barrows	Winchester
31	Charles Harold Berry	Belmont
41	Edward Lindley Bowles	Wellesley
50	Gordon Stanley Brown	Cambridge
25	Vannevar Bush	Washington, D. C.
49	Arthur Casagrande	Cambridge
48	John Chipman	Winchester
49	Edward Lull Cochrane	Cambridge
50	Morris Cohen	Cambridge
45	Hardy Cross	New Haven, Conn.
34	Otto Gustav Colbiornsen Dahl	Boston
34	Chester Laurens Dawes	Cambridge
34	Jacob Pieter Den Hartog	Wellesley Hills
43	Bradley Dewey	Cambridge

20	Theodore Harwood Dillon	Washington, D. C.
41	Charles Stark Draper	Newton
22	Gano Dunn	New York, N. Y.
21	William Frederick Durand	Brooklyn, N. Y.
46	Howard Wilson Emmons	Sudbury
27	Gordon Maskew Fair	Cambridge
48	Ivan Alexander Getting	Belmont
32	Glennon Gilboy	Lincoln
48	Edwin Richard Gilliland	Arlington
32	Albert Haertlein	Watertown
50	William Rede Hawthorne	Cambridge, England
40	Harold Locke Hazen	Belmont
44	Arthur Robert von Hippel	Weston
36	Murray Philip Horwood	Cambridge
48	Hoyt Clarke Hottel	Winchester
34	Jerome Clarke Hunsaker	Boston
49	Arthur Thomas Ippen	Cambridge
23	James Robertson Jack	Watertown
01	Lewis Jerome Johnson	Cambridge
48	Theodore von Kármán	Azusa, Cal.
37	Joseph Henry Keenan	Belmont
50	Robert Victor Kleinschmidt	Cambridge
23	William Henry Lawrence	Jamaica Plain
38	John Moyes Lessells	Brookline
48	William Henry McAdams	Newton
37	Charles Winters MacGregor	Philadelphia, Pa.
12	Lionel Simeon Marks	Cambridge
44	Richard von Mises	Cambridge
49	John Torrey Norton	Cambridge
20	Frederick Law Olmsted	Elkton, Md.
51	Egon Orowan	Cambridge
28	Langdon Pearse	Chicago, Ill.
13	Harold Pender	Wynnewood, Pa.
30	Greenleaf Whittier Pickard	Newton Center
51	William Prager	Providence, R. I.
41	Reinhold Rüdenberg	Belmont
48	Thomas Kilgore Sherwood	Wellesley
50	Cyril Stanley Smith	Chicago, Ill.
39	C. Richard Soderberg	Weston
14	Charles Milton Spofford	Boston
49	Charles Fayette Taylor	Cambridge
49	Edward Story Taylor	Cambridge
[28] 44	Karl Terzaghi	Winchester
50	John George Trump	Cambridge
49	Hsue-Shen Tsien	Pasadena, Cal.
23	Edward Pearson Warner	Montreal, Que.

51	Harold Christian Weber	Milton
48	Walter Gordon Whitman	Washington, D. C.
45	John Benson Wilbur	Belmont
40	John Wulff	Cambridge
41	Vladimir Kosma Zworykin	Princeton, N. J.

CLASS II—NATURAL AND PHYSIOLOGICAL SCIENCES—253

SECTION 1—*Geology, Mineralogy, and Physics of the Globe*—46

41	Alan Mara Bateman	New Haven, Conn.
46	Roland Frank Beers	Dallas, Texas
38	Marland Pratt Billings	Wellesley
21	Norman Levi Bowen	Washington, D. C.
49	Wilmot Hyde Bradley	Washington, D. C.
33	Charles Franklin Brooks	Milton
49	Walter Herman Bucher	New York, N. Y.
48	Arthur Francis Buddington	Princeton, N. J.
45	Martin Julian Buerger	Lincoln
33	Frank Morton Carpenter	Lexington
51	Everette Lee DeGolyer	Dallas, Texas
50	Carl Owen Dunbar	New Haven, Conn.
51	(William) Maurice Ewing	Palisades, N. Y.
49	Clifford Frondel	Cambridge
42	Russell Gibson	Belmont
49	James Gilluly	Los Angeles, Cal.
14	Louis Caryl Graton	Cambridge
17	Herbert Ernest Gregory	Honolulu, T. H.
50	Beno Gutenberg	Pasadena, Cal.
50	Donnel Foster Hewett	Pasadena, Cal.
46	Cornelius Searle Hurlbut, Jr.	Belmont
44	Columbus O'Donnell Iselin	Woods Hole
02	Thomas Augustus Jaggar	Honolulu, T. H.
48	Adolph Knopf	Stanford, Cal.
25	Esper Signius Larsen, Jr.	Arlington, Va.
15	Andrew Cowper Lawson	Berkeley, Cal.
16	Charles Kenneth Leith	Madison, Wis.
50	Arville Irving Levorsen	Tulsa, Okla.
31	George Francis McEwen	La Jolla, Cal.
49	Hugh Exton McKinstry	Cambridge
27	Donald Hamilton McLaughlin	San Francisco, Cal.
25	Kirtley Fletcher Mather	Newton Center
35	Warren Judson Mead	Belmont
17	William John Miller	Stockton, Cal.
32	Frederick Kuhne Morris	Montgomery, Ala.
34	Walter Harry Newhouse	Chicago, Ill.
50	Thomas Brennan Nolan	Washington, D. C.
22	Austin Flint Rogers	Palo Alto, Cal.

34	Carl-Gustaf Arvid Rossby	Chicago, Ill.
19	Waldemar Theodore Schaller	Washington, D. C.
48	George Gaylord Simpson	New York, N. Y.
45	Henry Crosby Stetson	Belmont
44	Harald Ulrik Sverdrup	Oslo, Norway
17	Thomas Wayland Vaughan	Washington, D. C.
35	Derwent Stainthorpe Whittlesey	Cambridge
15	Frederick Eugene Wright	Washington, D. C.

CLASS II, SECTION 2—*Botany*—42

30	LeRoy Abrams	Palo Alto, Cal.
34	Edgar Anderson	St. Louis, Mo.
15	Irving Widmer Bailey	Cambridge
00	Liberty Hyde Bailey	Ithaca, N. Y.
50	Elso Sterrenberg Barghoorn	Cambridge
49	Lawrence Rogers Blinks	Pacific Grove, Cal.
98	Douglas Houghton Campbell	Palo Alto, Cal.
48	Edward Sears Castle	Cambridge
49	Jens Christian Clausen	Stanford, Cal.
46	Ralph Erskine Cleland	Bloomington, Ind.
50	Lincoln Constance	Berkeley, Cal.
16	Bradley Moore Davis	Portland, Ore.
35	Bernard Ogilvie Dodge	New York, N. Y.
41	Arthur Johnson Eames	Ithaca, N. Y.
49	Katherine Esau	Davis, Cal.
12	Alexander William Evans	New Haven, Conn.
50	Adriance Sherwood Foster	Berkeley, Cal.
44	Paul Rupert Gast	Weston
27	Ivan Murray Johnston	Jamaica Plain
34	Donald Forsha Jones	New Haven, Conn.
40	Paul Christoph Mangelsdorf	Cambridge
10	Winthrop John Vanleuven Osterhout	New York, N. Y.
27	George James Peirce	Palo Alto, Cal.
49	Kenneth Bryan Raper	Peoria, Ill.
44	Hugh Miller Raup	Petersham
48	William Jacob Robbins	New York, N. Y.
49	Reed Clark Rollins	Cambridge
34	Edmund Ware Sinnott	New Haven, Conn.
44	Albert Charles Smith	Washington, D. C.
34	Gilbert Morgan Smith	Palo Alto, Cal.
45	Herman Augustus Spoehr	Palo Alto, Cal.
49	Lewis John Stadler	Columbia, Mo.
23	Elvin Charles Stakman	St. Paul, Minn.
48	William Randolph Taylor	Ann Arbor, Mich.
38	Kenneth Vivian Thimann	Cambridge

50	Cornelis Bernardus Van Niel	Pacific Grove, Cal.
49	Selman Abraham Waksman	New Brunswick, N. J.
48	Frits Warmolt Went	Pasadena, Cal.
22	William Henry Weston, Jr.	Winchester
32	Ralph Hartley Wetmore	Cambridge
50	Philip Rodney White	Bar Harbor, Me.
50	William Lawrence White	Cambridge

CLASS II, SECTION 3—*Zoology and Physiology*—85

50	Warder Clyde Allee	Gainesville, Fla.
49	Edwin Bennett Astwood	Boston
22	Nathan Banks	Holliston
33	Philip Bard	Baltimore, Md.
09	Francis Gano Benedict	Machiasport, Me.
11	Henry Bryant Bigelow	Concord
14	Robert Payne Bigelow	Brookline
35	Charles Henry Blake	Lincoln
20	William T. Bovie	Fairfield, Me.
24	Edward Allen Boyden	Minneapolis, Minn.
16	John Lewis Bremer	Boston
28	John Wymond Miller Bunker	Belmont
00	William Ernest Castle	Berkeley, Cal.
29	Lemuel Roscoe Cleveland	Jamaica Plain
26	Edwin Joseph Cohn	Cambridge
14	Edwin Grant Conklin	Princeton, N. J.
23	Manton Copeland	Brunswick, Me.
27	William John Crozier	Belmont
29	Hallowell Davis	St. Louis, Mo.
33	Alden Benjamin Dawson	Belmont
49	Edward Wheeler Dempsey	St. Louis, Mo.
25	Samuel Randall Detwiler	New York, N. Y.
43	David Bruce Dill	Army Medical Center, Md.
48	Edward Adelbert Doisy	St. Louis, Mo.
50	Leslie Clarence Dunn	New York, N. Y.
25	Herbert McLean Evans	Berkeley, Cal.
48	Wallace Osgood Fenn	Rochester, N. Y.
34	Cyrus Hartwell Fiske	Lexington
34	John Farquhar Fulton	New Haven, Conn.
48	Herbert Spencer Gasser	New York, N. Y.
47	Arnold Lucius Gesell	New Haven, Conn.
50	Roy Orval Greep	Boston
31	William King Gregory	New York, N. Y.
48	Edmund Newton Harvey	Princeton, N. J.
36	Frederick Lee Hisaw	Belmont
29	Leigh Hoadley	Cambridge
34	Hudson Hoagland	Shrewsbury

24	Samuel Jackson Holmes	Berkeley, Cal.
28	Roy Graham Hoskins	Waban
49	George Evelyn Hutchinson	New Haven, Conn.
51	Edward Calvin Kendall	Princeton, N. J.
49	Otto Krayer	Cambridge
44	Eugene Markley Landis	Brookline
14	Ralph Stayner Lillie	Chicago, Ill.
49	Cyril Norman Hugh Long	New Haven, Conn.
17	Richard Swann Lull	New Haven, Conn.
43	Brenton Reid Lutz	Melrose
27	Axel Leonard Melander	Riverside, Cal.
51	Charles William Metz	Philadelphia, Pa.
35	Karl Friedrich Meyer	San Francisco, Cal.
21	Gerrit Smith Miller	Washington, D. C.
50	Carl Richard Moore	Chicago, Ill.
42	Hermann Joseph Muller	Bloomington, Ind.
49	John Spangler Nicholas	New Haven, Conn.
95	George Howard Parker	Cambridge
46	James Lee Peters	Harvard
21	Henry Augustus Pilsbry	Philadelphia, Pa.
39	Gregory Pincus	Worcester
50	Harold Henry Plough	Amherst
27	Frederick Haven Pratt	Wellesley Hills
09	Herbert Wilbur Rand	Cambridge
32	David Rapport	Cambridge
23	Alfred Clarence Redfield	Woods Hole
34	Alfred Newton Richards	Bryn Mawr, Pa.
34	Oscar Riddle	Plant City, Fla.
46	Kenneth David Roeder	Concord
37	Alfred Sherwood Romer	Cambridge
25	Alexander Grant Ruthven	Ann Arbor, Mich.
51	Karl Patterson Schmidt	Chicago, Ill.
41	Francis Otto Schmitt	Belmont
49	Homer William Smith	New York, N. Y.
49	Tracy Morton Sonneborn	Bloomington, Ind.
49	Alfred Henry Sturtevant	Pasadena, Cal.
48	Hubert Bradford Vickery	New Haven, Conn.
48	George Wald	Belmont
45	John Henry Welsh	Cambridge
15	Arthur Wisswald Weysse	Woburn
51	Carroll Milton Williams	Cambridge
49	James Walter Wilson	Providence, R. I.
38	George Bernays Wislocki	Milton
48	Sewall Wright	Chicago, Ill.
50	Ralph Walter Graystone Wyckoff	Bethesda, Md.
33	Jeffries Wyman, Jr.	Chestnut Hill

45	Leland Clifton Wyman	Jamaica Plain
15	Robert Mearns Yerkes	New Haven, Conn.

CLASS II, SECTION 4—*Medicine and Surgery*—80

41	Fuller Albright	Brookline
47	Arthur Wilburn Allen	Brookline
32	Joseph Charles Aub	Belmont
36	Oswald Theodore Avery	Nashville, Tenn.
29	James Bourne Ayer	Milton
(28) 32	Franklin Greene Balch	Jamaica Plain
41	Walter Bauer	Waban
47	David Lawrence Belding	Hingham
50	George Packer Berry	Boston
48	Francis Gilman Blake	Washington, D. C.
31	George Blumer	San Marino, Cal.
49	Herrman Ludwig Blumgart	Boston
43	Arlie Vernon Bock	Cambridge
36	Charles Sidney Burwell	Brookline
48	Allan Macy Butler	Boston
31	William Bosworth Castle	Brookline
30	David Cheever	Boston
48	Edward Delos Churchill	Belmont
42	William Irving Clark	Worcester
49	David Glendenning Cogan	Cambridge
21	Rufus Cole	Mount Kisco, N. Y.
50	Oliver Cope	Boston
50	Lewis Dexter	Boston
31	Eugene Floyd DuBois	New York, N. Y.
50	Geoffrey Edsall	Bethesda, Md.
46	John Franklin Enders	Boston
49	Sidney Farber	Cambridge
45	James Morison Faulkner	Brookline
44	Maxwell Finland	Squantum
33	Reginald Fitz	Brookline
27	James Lawder Gamble	Brookline
22	Joseph Lincoln Goodale	Ipswich
49	Harry Sylvestre Nutting Greene	New Haven, Conn.
49	Robert Edward Gross	Boston
21	Ross Granville Harrison	New Haven, Conn.
51	Philip Showalter Hench	Rochester, Minn.
49	Arthur Tremain Hertig	Cambridge
47	Sanford Burton Hooker	Bradford, Vt.
49	Charles Brenton Huggins	Chicago, Ill.
33	Edgar Erskine Hume	Frankfort, Ky.
34	Henry Jackson, Jr.	Chestnut Hill
46	Charles Alderson Janeway	Weston

12 Elliott Proctor Joslin....................................Boston
43 Chester Scott Keefer....................................Brookline
23 Roger Irving Lee ....Brookline
42 Samuel Albert Levine..............................Newton Center
29 Edwin Allen Locke....................................Wilton, N. H.
49 Robert Frederick Loeb............................New York, N. Y.
28 Warfield Theobald Longcope........................Baltimore, Md.
40 William de Berniere MacNider...................Chapel Hill, N. C.
44 William Malamud....................................Boston
[27] 50 James Howard Means..............................Boston
34 Leroy Matthew Simpson Miner.........................Newtonville
28 William Lorenzo Moss..................................Athens, Ga.
28 John Howard Mueller..............................West Roxbury
50 Ira Theodore Nathanson....................................Boston
51 John Rodman Paul..............................New Haven, Conn.
27 Joseph Hersey Pratt....................................Brookline
50 Samuel Proger....................................Boston
35 Tracy Jackson Putnam..........................Beverly Hills, Cal.
34 William Carter Quinby....................................Brookline
47 Francis Minot Rackemann..............................Boston
48 John Rock....................................Roslindale
34 Arthur Hiler Ruggles..............................Providence, R. I.
39 William Thomas Salter..........................New Haven, Conn.
33 George Cheever Shattuck..............................Brookline
47 James Stevens Simmons....................................Boston
47 Richard Mason Smith....................................Boston
50 John Crayton Snyder....................................Boston
30 Torald Hermann Sollmann..............................Cleveland, O.
47 Merrill Clary Sosman..............................Chestnut Hill
51 Nathan Bill Talbot....................................Boston
46 Siegfried Josef Thannhauser..............................Brookline
44 George Widmer Thorn....................................Cambridge
14 Ernest Edward Tyzzer....................................Wakefield
14 Frederick Herman Verhoeff..............................Brookline
47 Shields Warren....................................West Newton
27 Joseph Treloar Wearn..............................Cleveland, O.
40 Paul Dudley White....................................Brookline
12 Simeon Burt Wolbach..............................South Sudbury

CLASS III—The Social Arts—211

Section 1—*Jurisprudence*—37

(24) 32 Francis Noyes Balch..............................Jamaica Plain
36 Stoughton Bell....................................Cambridge
36 Claude Raymond Branch....................................Boston

50	Zechariah Chafee, Jr.	Cambridge
48	Charles Allerton Coolidge	Belmont
49	John Cobb Cooper	Princeton, N. J.
33	John Dickinson	Philadelphia, Pa.
38	Robert Gray Dodge	Boston
40	Lon Louvois Fuller	Cambridge
39	Herbert Funk Goodrich	Philadelphia, Pa.
33	Theodore Francis Green	Providence, R. I.
38	Frank Washburn Grinnell	Boston
41	Erwin Nathaniel Griswold	Belmont
39	John Loomer Hall	Boston
39	Augustus Noble Hand	New York, N. Y.
33	Learned Hand	New York, N. Y.
39	Albert James Harno	Urbana, Ill.
49	Charles Antone Horsky	Washington, D. C.
47	Mark DeWolfe Howe	Cambridge
38	Melvin Maynard Johnson	Brookline
49	Phillips Ketchum	Cambridge
38	James McCauley Landis	New York, N. Y.
32	Sayre Macneil	Los Angeles, Cal.
32	Calvert Magruder	Cambridge
50	Thomas Harrison Mahony	Boston
51	Harold Medina	Foley Square, N. Y.
31	William DeWitt Mitchell	New York, N. Y.
31	Edmund Morris Morgan	Nashville, Tenn.
47	John Lord O'Brian	Washington, D. C.
36	Henry Parkman	Boston
01	George Wharton Pepper	Philadelphia, Pa.
11	Roscoe Pound	Watertown
36	Stanley Elroy Qua	Lowell
32	Francis Bowes Sayre	Washington, D. C.
21	Austin Wakeman Scott	Cambridge
39	Thomas Walter Swan	New York, N. Y.
43	Charles Edward Wyzanski, Jr.	Cambridge

CLASS III, SECTION 2—*Government, International Law, and Diplomacy*—34

36	Howard Landis Bevis	Columbus, O.
51	Ralph Johnson Bunche	United Nations, N. Y.
46	Harvey Hollister Bundy	Boston
40	Robert Granville Caldwell	Buenos Aires, Argentina
32	William Richards Castle, Jr.	Washington, D. C.
48	William Lockhart Clayton	Houston, Tex.
33	Robert Treat Crane	Baltimore, Md.
50	Saville Rogers Davis	Boston
46	John Sloan Dickey	Hanover, N. H.
50	Paul Howard Douglas	Washington, D. C.

50	Merle Fainsod	Cambridge
27	William Cameron Forbes	Norwood
44	Carl Joachim Friedrich	Concord
50	James William Fulbright	Washington, D. C.
34	Edgar Stephenson Furniss	New Haven, Conn.
50	John Merriman Gaus	Cambridge
49	Leland Matthew Goodrich	New York, N. Y.
32	Joseph Clark Grew	Washington, D. C.
50	Christian Archibald Herter	Washington, D. C.
41	Hajo Holborn	Hamden, Conn.
27	Arthur Norman Holcombe	Cambridge
32	Philip Carryl Jessup	New York, N. Y.
49	Walter Lippmann	Washington, D. C.
32	Charles Edward Merriam	Chicago, Ill.
13	William Bennett Munro	Pasadena, Cal.
47	William Phillips	North Beverly
41	Gaetano Salvemini	Cambridge
46	Robert Burgess Stewart	Winchester
44	Sarah Wambaugh	Cambridge
47	Sumner Welles	Oxon Hill, Md.
32	William Franklin Willoughby	Washington, D. C.
48	Howard Eugene Wilson	New York, N. Y.
27	Quincy Wright	Chicago, Ill.
33	Henry Aaron Yeomans	Harvard

CLASS III, SECTION 3—*Economics and Sociology*—58

36	James Waterhouse Angell	New York, N. Y.
47	George Pierce Baker	Boston
48	Richard Mervin Bissell, Jr.	Washington, D. C.
36	James Cummings Bonbright	New York, N. Y.
43	Augusta Fox Bronner (Mrs. William Healy)	Clearwater, Fla.
44	Douglass Vincent Brown	Brookline
46	Theodore Henry Brown	Cambridge
34	John Maurice Clark	Westport, Conn.
28	Arthur Harrison Cole	Cambridge
51	James Frederic Dewhurst	New York, N. Y.
32	Arthur Stone Dewing	Newton
41	Carl Rupp Doering	Norman, Okla.
32	Wallace Brett Donham	Cambridge
36	Fred Rogers Fairchild	Hamden, Conn.
34	Ralph Evans Freeman	Cambridge
33	Sheldon Glueck	Cambridge
50	Carter Goodrich	New York, N. Y.
34	Robert Murray Haig	New York, N. Y.
41	Earl Jefferson Hamilton	Evanston, Ill.
45	Seymour Edwin Harris	West Acton

51 Caryl Parker Haskins.............................New York, N. Y.
49 (Edward) Pendleton Herring......................New York, N. Y.
48 Bishop Carleton Hunt..................................Norwell
34 Frank Hyneman Knight............................Chicago, Ill.
51 Paul Felix Lazarsfeld...........................New York, N. Y.
34 Robert Morison MacIver..........................New York, N. Y.
32 Walter Wallace McLaren.....................Englewood Cliffs, N. J.
45 William Rupert Maclaurin............................Cambridge
36 Malcolm Perrine McNair..............................Cambridge
32 Leon Carroll Marshall...........................Chevy Chase, Md.
50 Mark Arthur May.................................New Haven, Conn.
34 Richard Stockton Meriam.........................South Lincoln
50 Robert King Merton..............................New York, N. Y.
32 Frederick Cecil Mills...........................New York, N. Y.
34 Arthur Eli Monroe...................................Cambridge
34 Edwin Griswold Nourse...........................Chevy Chase, Md.
32 William Fielding Ogburn.........................Chicago, Ill.
45 Talcott Parsons.....................................Belmont
42 William Andrew Paton............................Ann Arbor, Mich.
47 Fritz Jules Roethlisberger..........................Cambridge
37 Clyde Orval Ruggles.............................Dayton, Ohio
42 Paul Anthony Samuelson..............................Belmont
36 Thomas Henry Sanders................................Cambridge
43 Benjamin Morris Selekman............................Cambridge
31 Pitirim Alexandrovich Sorokin.......................Winchester
31 Oliver Mitchell Wentworth Sprague...................Cambridge
46 Harold Walter Stoke.............................Seattle, Wash.
50 Samuel Andrew Stouffer..............................Cambridge
49 Philip Taft.....................................Providence, R. I.
37 Harry Rudolph Tosdal................................Belmont
31 Donald Skeele Tucker................................Belmont
41 Robert Ulich..Cambridge
34 Jacob Viner.....................................Princeton, N. J.
38 T[homas] North Whitehead............................Cambridge
32 John Henry Williams.................................Cambridge
36 Joseph Henry Willits............................New York, N. Y.
34 Leo Wolman......................................New York, N. Y.
34 Carle Clark Zimmerman...............................Winchester

CLASS III, SECTION 4—*Administration and Affairs*—82

(25) 32 Charles Francis Adams...........................Concord
46 Chester M. Alter....................................West Newton
51 Roger Nash Baldwin..............................New York, N. Y.
39 Chester Irving Barnard..........................New York, N. Y.
(25) 32 Charles Foster Batchelder.................Peterborough, N. H.
49 Bancroft Beatley....................................Boston

44	Adolf Augustus Berle, Jr.	New York, N. Y.
50	Barry Bingham	Louisville, Ky.
49	S(amuel) Bruce Black	Boston
49	Lloyd DeW. Brace	Boston
49	Detlev Wulf Bronk	Baltimore, Md.
49	Oliver Ellsworth Buckley	New York, N. Y.
50	John Ely Burchard	Cambridge
45	Edwin Sharp Burdell	New York, N. Y.
50	Victor Lloyd Butterfield	Middletown, Conn.
41	Godfrey Lowell Cabot	Boston
47	Thomas Dudley Cabot	Weston
48	Erwin Dain Canham	Newton
47	William Henry Claflin, Jr.	Belmont
49	Paul Foster Clark	Boston
48	Charles Woolsey Cole	Amherst
43	Ada Louise Comstock (Mrs. Wallace Notestein)	New Haven, Conn.
50	William Terry Couch	Chicago, Ill.
48	Robert Cutler	Boston
42	Donald Kirk David	Boston
32	Henry Sturgis Dennison	Framingham
50	Charles Dollard	New York, N. Y.
46	David Frank Edwards	Cambridge
48	Carl Stephens Ell	Newton
49	John Wells Farley	Boston
(28) 32	William Lusk Webster Field	Milton
39	Ralph Edward Flanders	Springfield, Vt.
38	Horace Sayford Ford	Belmont
44	Raymond Blaine Fosdick	New York, N. Y.
51	Clinton Strong Golden	Solebury, Pa.
48	Francis Calley Gray	Boston
51	Crawford Hallock Greenewalt	Wilmington, Del.
50	Alan Gregg	New York, N. Y.
51	Alfred Whitney Griswold	New Haven, Conn.
50	William Averell Harriman	Washington, D. C.
49	Harold Daniel Hodgkinson	Boston
44	Ernest Martin Hopkins	Hanover, N. H.
51	William Vermillion Houston	Houston, Texas
48	Carl Tilden Keller	Boston
34	Henry Plimpton Kendall	Sharon
44	James Rhyne Killian, Jr.	Wellesley Hills
39	Morris Evans Leeds	Philadelphia, Pa.
48	David Eli Lilienthal	Washington, D. C.
34	Clarence Cook Little	Bar Harbor, Me.
47	Ralph Lowell	Westwood
49	Louis Martin Lyons	Cambridge
36	Dumas Malone	Charlottesville, Va.

42 Daniel L. Marsh..........Boston
45 Keyes DeWitt Metcalf..........Belmont
45 Henry Allen Moe..........New York, N. Y.
02 Herbert Putnam..........Washington, D. C.
51 William McNear Rand..........Lincoln
50 Helen Rogers Reid..........New York, N. Y.
49 Arthur Grinnell Rotch..........Boston
51 Abram Leon Sachar..........Waltham
34 Erwin Haskell Schell..........Cambridge
38 Charles Seymour..........New Haven, Conn.
35 Henry Lee Shattuck..........Boston
37 Henry Southworth Shaw..........Melrose
50 George Nauman Shuster..........New York, N. Y.
48 Alfred Pritchard Sloan, Jr..........New York, N. Y.
49 George A. Sloan..........New York, N. Y.
(28) 32 Payson Smith..........Orono, Me.
46 Huntley Nowell Spaulding..........Rochester, N. H.
33 Albert Warren Stearns..........Billerica
50 Arthur Hays Sulzberger..........New York, N. Y.
50 William Webster..........Washington, D. C.
51 Leonard Dupee White..........Chicago, Ill.
51 Walter Muir Whitehill..........Boston
49 Carroll Louis Wilson..........Washington, D. C.
44 Charles Edward Wilson..........New York, N. Y.
50 Thomas James Wilson..........Cambridge
41 Laurence Leathe Winship..........South Sudbury
50 Benjamin Fletcher Wright..........Northampton
48 Henry Merritt Wriston..........Providence, R. I.
(25) 32 Benjamin Loring Young..........Weston
39 Owen D. Young..........Van Hornesville, N. Y.

CLASS IV—THE HUMANITIES—212

SECTION 1—*Theology, Philosophy, and Psychology*—48

33 John Gilbert Beebe-Center..........Swampscott
38 Julius Seelye Bixler..........Waterville, Me.
46 Brand Blanshard..........New Haven, Conn.
24 Edwin Garrigues Boring..........Cambridge
28 Edgar Sheffield Brightman..........Newton
31 Henry Addington Bruce..........Cambridge
49 Millar Burrows..........New Haven, Conn.
32 Leonard Carmichael..........Tufts College
48 Rudolph Carnap..........Chicago, Ill.
36 Robert Pierce Casey..........Providence, R. I.
33 Curt John Ducasse..........Providence, R. I.
43 Angus Dun..........Washington, D. C.

48	Frederick May Eliot	Cambridge
38	James Everett Frame	Princeton, N. J.
37	Clarence Henry Graham	New York, N. Y.
45	Edwin Ray Guthrie	Seattle, Wash.
32	William Healy	Clearwater, Fla.
35	Clark Leonard Hull	New Haven, Conn.
28	Albert Cornelius Knudson	Cambridge
50	John LaFarge	New York, N. Y.
32	Karl Spencer Lashley	Orange Park, Fla.
29	Clarence Irving Lewis	Lexington
42	Richard Peter McKeon	Chicago, Ill.
48	Arthur Edward Murphy	Ithaca, N. Y.
50	Gardner Murphy	New York, N. Y.
35	Henry Alexander Murray, Jr.	Boston
47	Norman Burdett Nash	Boston
32	Arthur Darby Nock	Cambridge
51	Filmer Sears Cuckow Northrop	New Haven, Conn.
28	Johnson O'Connor	Boston
17	Charles Edwards Park	Boston
33	Carroll Cornelius Pratt	Princeton, N. J.
49	Willard Van Orman Quine	Cambridge
48	Hans Reichenbach	Los Angeles, Cal.
50	Herbert Wallace Schneider	New York, N. Y.
31	Henry Knox Sherrill	New York, N. Y.
27	Willard Learoyd Sperry	Cambridge
51	Walter Terence Stace	Princeton, N. J.
29	Russell Henry Stafford	Hartford, Conn.
46	Stanley Smith Stevens	Cambridge
48	Alfred Tarski	Berkeley, Cal.
45	Charles Lincoln Taylor, Jr.	Cambridge
34	Lewis Madison Terman	Palo Alto, Cal.
27	Louis Leon Thurstone	Chicago, Ill.
50	Paul Johannes Tillich	New York, N. Y.
49	Edward Chace Tolman	Berkeley, Cal.
17	John Broadus Watson	Woodbury, Conn.
35	Robert Sessions Woodworth	New York, N. Y.

CLASS IV, SECTION 2—*History, Archaeology, and Anthropology*—49

41	Warren Ortman Ault	Waban
28	James Phinney Baxter, 3d	Williamstown
50	Herbert Bloch	Cambridge
46	Clarence Saunders Brigham	Worcester
42	Henry Joel Cadbury	Cambridge
50	Helen Maud Cam	Cambridge
34	Clarence Gordon Campbell	New York, N. Y.

43	Carleton Stevens Coon	Devon, Pa.
49	Merle (Eugene) Curti	Madison, Wis.
44	William Bell Dinsmoor	New York, N. Y.
51	John Dollard	New Haven, Conn.
51	Douglas Southall Freeman	Richmond, Va.
38	Claude Moore Fuess	Chestnut Hill
50	Hetty Goldman	Princeton, N. J.
50	Louis Gottschalk	Chicago, Ill.
49	Mason Hammond	Cambridge
49	James Blaine Hedges	Providence, R. I.
43	Hugh O'Neill Hencken	Chestnut Hill
14	Bert Hodge Hill	Athens, Greece
27	Earnest Albert Hooton	Cambridge
33	Halford Lancaster Hoskins	Washington, D. C.
47	Wilbur Kitchener Jordan	Cambridge
50	Michael Karpovich	Cambridge
44	Clyde Kay Maben Kluckhohn	Cambridge
12	Alfred Louis Kroeber	Berkeley, Cal.
44	Ambrose Lansing	New York, N. Y.
49	Kenneth Scott Latourette	New Haven, Conn.
32	Waldo Gifford Leland	Newton
41	William E. Lingelbach	Philadelphia, Pa.
50	Ralph Linton	New Haven, Conn.
48	Margaret Mead	New York, N. Y.
38	Stewart Mitchell	Boston
15	Samuel Eliot Morison	Boston
51	George Peter Murdock	New Haven, Conn.
46	Otto Eduard Neugebauer	Providence, R. I.
51	Allan Nevins	New York, N. Y.
34	Robert Henry Pfeiffer	Cambridge
50	Conyers Read	Philadelphia, Pa.
50	Robert Redfield	Chicago, Ill.
34	David Moore Robinson	University, Miss.
23	Michael Ivanovich Rostovtzeff	New Haven, Conn.
38	Bernadotte Everly Schmitt	Alexandria, Va.
36	Donald Scott	Cambridge
49	Richard Harrison Shryock	Baltimore, Md.
26	Herbert Joseph Spinden	Carmel, N. Y.
32	Charles Holt Taylor	Cambridge
51	Lily Ross Taylor	Bryn Mawr, Pa.
11	Alfred Marston Tozzer	Cambridge
39	Henry Rouse Viets	Brookline

CLASS IV, SECTION 3—*Philology*—46

48	Bernard Bloch	New Haven, Conn.
33	Campbell Bonner	Ann Arbor, Mich.

21	Carl Darling Buck	Chicago, Ill.
49	Oscar James Campbell	New York, N. Y.
48	Yuen Ren Chao	Berkeley, Cal.
20	Walter Eugene Clark	La Jolla, Cal.
46	George Raleigh Coffman	Newton
32	Ronald Salmon Crane	Chicago, Ill.
44	Henry Grattan Doyle	Washington, D. C.
20	Franklin Edgerton	New Haven, Conn.
40	Serge Elisséeff	Cambridge
14	Jeremiah Denis Mathias Ford	Cambridge
16	Louis Herbert Gray	New York, N. Y.
25	William Chase Greene	Cambridge
13	Charles Burton Gulick	Scarsdale, N. Y.
31	Raymond Dexter Havens	Baltimore, Md.
18	George Lincoln Hendrickson	New Haven, Conn.
40	Werner Wilhelm Jaeger	Watertown
50	Roman Jakobson	Cambridge
32	(Ralph) Hayward Keniston	Ann Arbor, Mich.
34	Roland Grubb Kent	Wynnewood, Pa.
33	Hans Kurath	Ann Arbor, Mich.
39	Henry Carrington Lancaster	Baltimore, Md.
50	Harry Tuchman Levin	Cambridge
33	Ivan Mortimer Linforth	Berkeley, Cal.
44	Elias Avery Lowe	Princeton, N. J.
35	Benjamin Dean Meritt	Princeton, N. J.
28	William Albert Nitze	Los Angeles, Cal.
32	George Rapall Noyes	Berkeley, Cal.
33	Howard Rollin Patch	Northampton
11	Fred Norris Robinson	Cambridge
38	Hyder Edward Rollins	Cambridge
35	Henry Arthur Sanders	Ann Arbor, Mich.
43	Jean Joseph Seznec	Cambridge
45	George Wiley Sherburn	Cambridge
50	Charles Southward Singleton	Cambridge
45	Taylor Starck	Cambridge
49	Archer Taylor	Berkeley, Cal.
32	William Thomson	South Lincoln
11	Charles Cutler Torrey	New Haven, Conn.
47	William Freeman Twaddell	Providence, R. I.
48	Berthold Louis Ullman	Chapel Hill, N. C.
51	Charles Frederick Voegelin	Bloomington, Ind.
30	Ernest Hatch Wilkins	Newton Center
33	Harry Austryn Wolfson	Cambridge
49	Louis Booker Wright	Washington, D. C.

CLASS IV, SECTION 4—*The Fine Arts and Belles Lettres*—69

43	Leonard Bacon	Peace Dale, R. I.
26	Frank Weston Benson	Salem
51	Leonard Bernstein	New York, N. Y.
50	E. Power Biggs	Cambridge
32	(William) Welles Bosworth	Locust Valley, N. Y.
49	Van Wyck Brooks	Bridgewater, Conn.
50	John Nicholas Brown	Providence, R. I.
47	Richard Burgin	Brookline
42	John Nash Douglas Bush	Cambridge
45	Samuel Chamberlain	Marblehead
32	Chalmers Dancy Clifton	New York, N. Y.
49	Robert Peter Tristram Coffin	Brunswick, Me.
32	Kenneth John Conant	Cambridge
46	William George Constable	Cambridge
51	Elizabeth Sprague Coolidge	Cambridge
51	Aaron Copland	Cambridge
49	Bernard Augustine DeVoto	Cambridge
47	John Roderigo Dos Passos	Westmoreland County, Va.
49	William Addison Dwiggins	Hingham
32	George Harold Edgell	Cambridge
21	William Emerson	Cambridge
50	Luther Harris Evans	Washington, D. C.
18	Edward Waldo Forbes	Cambridge
49	Esther Forbes	Worcester
31	Robert Frost	Ripton, Vt.
27	Wallace Goodrich	Manchester
44	Walter Gropius	Lincoln
48	Bartlett Harding Hayes	Andover
31	Robert Silliman Hillyer	Greenwich, Conn.
27	Charles Hopkinson	Manchester
12	Mark Antony DeWolfe Howe	Boston
38	Joseph Hudnut	Cambridge
18	Archer Milton Huntington	New York, N. Y.
50	William Mills Ivins, Jr.	Woodbury, Conn.
45	William Alexander Jackson	Cambridge
38	Howard Mumford Jones	Cambridge
42	Otto Kinkeldey	New York, N. Y.
49	Jack Levine	New York, N. Y.
47	Milton Edward Lord	Boxford
50	Archibald MacLeish	Cambridge
21	Charles Donagh Maginnis	Brookline
49	Thomas Mann	Pacific Palisades, Cal.
31	Paul Manship	New York, N. Y.
48	John Marin	Cliffside Park, N. J.
31	Daniel Gregory Mason	New York, N. Y.

31	Frank Jewett Mather	Princeton, N. J.
44	Charles Rufus Morey	Princeton, N. J.
47	Lewis Mumford	New York, N. Y.
50	Charles Munch	Boston
48	Thomas Munro	Cleveland, O.
31	Kenneth Ballard Murdock	Boston
48	Erwin Panofsky	Princeton, N. J.
(24) 32	Anthony John Philpott	Arlington
41	Walter Hamor Piston, Jr.	Belmont
49	Lucien Price	Boston
22	Paul Joseph Sachs	Cambridge
50	Charles Henry Sawyer	New Haven, Conn.
14	Ellery Sedgwick	Beverly
19	Henry Dwight Sedgwick	Dedham
48	Bruce Simonds	Hamden, Conn.
39	Francis Henry Taylor	New York, N. Y.
43	Randall Thompson	Cambridge
45	Martin Wagner	Cambridge
51	Robert Penn Warren	New Haven, Conn.
46	Edward Augustus Weeks, Jr.	Boston
39	Karl Ephraim Weston	Williamstown
51	George Frisbie Whicher	Amherst
51	Edgar Wind	Northampton
46	William Wilson Wurster	San Francisco, Cal.

FELLOWS EMERITI—56

CLASS I—Mathematical and Physical Sciences—11

Class I, Section 1—1

15	Frank Lauren Hitchcock	Locust Valley, N. Y.

Class I, Section 2—2

39	Lyman James Briggs	Washington, D. C.
12	Maurice deKay Thompson	Boston

Class I, Section 3—6

19	Arthur Alphonso Blanchard	Brookline
40	William Lloyd Evans	Columbus, O.
15	George Shannon Forbes	Cambridge
32	Kenneth Lamartine Mark	Boston
14	Samuel Cate Prescott	Brookline
19	Robert Seaton Williams	Belmont

CLASS I, SECTION 4—2

32	Ralph Restieaux Lawrence	Belmont
30	George Edmond Russell	Lexington

CLASS II—NATURAL AND PHYSIOLOGICAL SCIENCES—14

CLASS II, SECTION 1—5

09	Reginald Aldworth Daly	Cambridge
34	Sterling Price Fergusson	Milton
[03] 44	Charles Palache	Cambridge
17	Percy Edward Raymond	Lexington
11	Hervey Woodburn Shimer	Hingham

CLASS II, SECTION 2—3

40	Albert Francis Blakeslee	Northampton
29	Joseph Horace Faull	Cambridge
21	Elmer Drew Merrill	Jamaica Plain

CLASS II, SECTION 3—3

15	Charles Thomas Brues	Newtonville
22	Thorne Martin Carpenter	Foxboro
15	Alexander Forbes	Milton

CLASS II, SECTION 4—3

25	Robert Bayley Osgood	Boston
32	Frederick Fuller Russell	Louisville, Ky.
30	Fritz Bradley Talbot	Wianno

CLASS III—THE SOCIAL ARTS—7

CLASS III, SECTION 1—1

(28) 32	Edmund Allen Whitman	Cambridge

CLASS III, SECTION 2—1

31	Sidney Bradshaw Fay	Cambridge

CLASS III, SECTION 3—3

32	William James Cunningham	Cambridge
32	Henry Wyman Holmes	Cambridge
33	Abbott Payson Usher	Salem

CLASS III, SECTION 4—2

35	Jerome Davis Greene	Cambridge
36	Clair Elsmere Turner	Arlington

CLASS IV—THE HUMANITIES—24

CLASS IV, SECTION 1—4

28	Walter Fenno Dearborn	Cambridge
30	William Henry Paine Hatch	Exeter, N. H.
21	William Ernest Hocking	Madison, N. H.
28	Henry Bradford Washburn	Cambridge

CLASS IV, SECTION 2—6

12	George Henry Chase	Cambridge
49	Gilbert Chinard	Princeton, N. J.
21	William Scott Ferguson	Cambridge
22	George La Piana	Wellesley
20	Charles Howard McIlwain	Auburndale
27	George Sarton	Cambridge

CLASS IV, SECTION 3—8

41	Giuseppe Antonio Borgese	Chicago, Ill.
38	Richmond Laurin Hawkins	Cambridge
17	William Guild Howard	Cambridge
32	Ernest Felix Langley	Cambridge
32	Arthur Stanley Pease	Cambridge
44	La Rue Van Hook	New York, N. Y.
33	George Benson Weston	Cambridge
39	William Hoyt Worrell	Melbourne Beach, Fla.

CLASS IV, SECTION 4—6

29	Charles Townsend Copeland	Cambridge
41	Archibald Thompson Davison	Lincoln
29	Edward Burlingame Hill	Francestown, N. H.
21	Chandler Rathfon Post	Cambridge
[35] 44	Walter Raymond Spalding	Cambridge
22	Charles Henry Conrad Wright	Cambridge

FOREIGN HONORARY MEMBERS—145

(Number limited to one hundred and fifty)

CLASS I—MATHEMATICAL AND PHYSICAL SCIENCES—40

SECTION 1—*Mathematics and Astronomy*—12

50	Henri Cartan	Paris
39	Arnaud Denjoy	Paris
34	Ronald Aylmer Fisher	Cambridge, England
20	Jacques Salomon Hadamard	Paris
27	Ejnar Hertzsprung	Leiden
45	Sir Harold Spencer Jones	Greenwich
46	Bertil Lindblad	Stockholm
46	Jan Hendrik Oort	Leiden
47	Meghnad N. Saha	Calcutta
47	G. A. Shajn	Simeis, U. S. S. R.
15	Charles Jean de la Vallée Poussin	Louvain
29	Hermann Weyl	Princeton, N. J.

CLASS I, SECTION 2—*Physics*—10

45	Niels Bohr	Copenhagen
50	Sir John Douglas Cockcroft	Harwell, England
50	Paul Adrien Maurice Dirac	Cambridge, England
24	Albert Einstein	Princeton, N. J.
29	James Franck	Chicago, Ill.
29	Abram F. Joffé	Leningrad
48	Max Felix Theodor von Laue	Berlin-Dahlem
50	Wolfgang Pauli	Zürich
50	Erwin Schrödinger	Dublin
47	Sir George Paget Thomson	London

CLASS I, SECTION 3—*Chemistry*—11

27	Peter Debye	Ithaca, N. Y.
50	George De Hevesy	Copenhagen
33	Jaroslav Heyrovsky	Prague
51	Sir Cyril (Norman) Hinshelwood	Oxford
33	Fritz Paneth	Durham
48	Sir Robert Robinson	Oxford
38	Leopold Ruzicka	Zürich
38	Nevil Vincent Sidgwick	Oxford
48	The Svedberg	Uppsala
49	Chaim Weizmann	Rehovoth, Israel
29	Heinrich Wieland	Munich

Class I, Section 4—*Technology and Engineering*—7

36	Edward Victor Appleton	London
46	Clarence Decatur Howe	Ottawa
34	Luigi Lombardi	Rome
50	Adolph Meyer	Baden, Switzerland
29	Ludwig Prandtl	Göttingen
29	Emil Probst	Oxford
31	Karl Willy Wagner	Friedrichsdorf

CLASS II—Natural and Physiological Sciences—31

Section 1—*Geology, Mineralogy, and Physics of the Globe*—5

36	Raoul Blanchard	Grenoble, France
29	Léon William Collet	Geneva
34	Arthur Holmes	Edinburgh
22	Emmanuel de Margerie	Paris
47	Wong Wen-hao	Nanking

Class II, Section 2—*Botany*—9

48	P. Boysen-Jensen	Copenhagen
46	Rudolf Florin	Stockholm
50	Hendrik Gunnar Lundegardh	Uppsala
47	P. Maheshwari	Dacca, India
42	Lorenzo Raimundo Parodi	Buenos Aires
47	Alf Erling Porsild	Ottawa
29	Otto Renner	Jena
35	Sir William Wright Smith	Edinburgh
51	Claude (Wilson) Wardlaw	Manchester

Class II, Section 3—*Zoology and Physiology*—12

38	Edgar Douglas Adrian	Cambridge, England
48	Charles Herbert Best	Toronto
20	Maurice Caullery	Paris
38	Emmanuel Fauré-Frémiet	Paris
51	Sir Charles (Robert) Harington	London
34	Archibald Vivian Hill	London
41	Bernado Alberto Houssay	Buenos Aires
48	Archibald Gowanlock Huntsman	Toronto
30	Louis Édouard Lapicque	Paris
50	Kaj Ulrik Linderström-Lang	Copenhagen
50	Rudolph Albert Peters	Oxford
50	Hugo Theorell	Stockholm

Class II, Section 4—*Medicine and Surgery*—5

39	Sir Aldo Castellani	Rome
27	Sir Henry Hallett Dale	London
33	Sir Arthur Keith	London
50	Wilder Graves Penfield	Montreal
18	Sir Charles Scott Sherrington	Cambridge, England

CLASS III—The Social Arts—35

Section 1—*Jurisprudence*—12

44	Victor Andres Belaunde	Lima
47	Paal Berg	Oslo
33	François Geny	Nancy
38	Arthur Lehman Goodhart	Oxford
39	Baron Greene (Wilfrid Arthur Greene)	Holmbury St. Mary, Surrey
33	Hans Kelsen	Berkeley, Cal.
38	Baron Macmillan (Hugh Pattison Macmillan)	Ewhurst, Surrey
33	Juljusz Makarewicz	Lwów
48	Léon Julliot de la Morandière	Paris
33	Giorgio Del Vecchio	Rome
38	Baron Wright (Robert Alderson Wright)	Burbage, Wilts
38	John C. H. Wu	Vatican City

Section 2—*Government, International Law, and Diplomacy*—11

48	José d'Almada	Lisbon
46	Winston Churchill	London
32	Paul Claudel	Paris
50	Herbert Vere Evatt	Canberra, Australia
50	Trygve Halvdan Lie	Lake Success, N. Y.
51	Charles Habib Malik	New York, N. Y.
50	Jawaharlal Nehru	New Delhi, India
32	Hu Shih	Peiping
50	Paul-Henri Spaak	Brussels
38	Kenzo Takayanagi	Tokyo
49	Sir Alfred Zimmern	Springfield, Mass.

Section 3—*Economics and Sociology*—8

32	Arthur Lyon Bowley	Haslemere, Surrey
39	Henry Clay	Oxford
44	Daniel Cosío Villegas	Mexico, D. F.
35	Luigi Einaudi	Rome
32	Ralph George Hawtrey	London

28	Arthur Cecil Pigou	Cambridge, England
32	Charles Rist	Versailles
45	Dennis Holme Robertson	Cambridge, England

SECTION 4—*Administration and Affairs*—4

33	Gösta A. Bagge	Stockholm
38	Heinrich Brüning	Cambridge, Mass.
45	Alfredo Lorenzo Palacios	Buenos Aires
38	B. Seebohm Rowntree	North Dean, Bucks

CLASS IV—THE HUMANITIES—39

SECTION 1—*Theology, Philosophy, and Psychology*—8

50	Karl Barth	Basle
28	Benedetto Croce	Naples
29	Étienne Gilson	Melun
28	Wolfgang Köhler	Swarthmore, Pa.
37	Henri Piéron	Paris
46	Francisco Romero	Buenos Aires
50	Sir William David Ross	Oxford
47	Godfrey Hilton Thomson	Edinburgh

SECTION 2—*History, Archaeology, and Anthropology*—9

33	Rafael Altamira y Crevea	Madrid
36	Marcel Aubert	Paris
50	George Norman Clark	Oxford
33	Friedrich Meinecke	Berlin-Dahlem
50	John Ernest Neale	London
40	Martin Persson Nilsson	Lund
51	Henry Ernest Sigerist	Pura, Switzerland
49	Arnold Joseph Toynbee	Oxford
31	George Macaulay Trevelyan	Cambridge, England

SECTION 3—*Philology*—10

44	Amado Alonso	Arlington, Mass.
47	Hamilton Alexander Rosskeen Gibb	Oxford
47	Joseph Klausner	Jerusalem
36	Paul Kretschmer	Vienna
32	Paul Mazon	Paris
17	Ramón Menéndez Pidal	Madrid
50	Roger Aubrey Baskerville Mynors	Cambridge, England
45	Tomás Navarro Tomás	New York, N. Y.
[12] 28	Hanns Oertel	Munich
32	Frederick William Thomas	Bodicote nr. Banbury

SECTION 4—*The Fine Arts and Belles Lettres*—12

40	Fernand Baldensperger	Paris
48	Jean-Marie Carré	Paris
40	Paul Hindemith	New Haven, Conn.
27	Gilbert Murray	Oxford
49	Oscar Niemeyer	Rio de Janeiro
31	Edgar Allison Peers	Liverpool
48	Jan Hendrik Scholte	Amsterdam
49	Levin Ludwig Schücking	Erlangen, Bavaria
41	Jean Julius Christian Sibelius	Helsinki
40	Igor Stravinsky	Hollywood, Cal.
50	Sir Stanley Unwin	London
50	Rebecca West	Ibstone, Bucks, England

ALPHABETICAL INDEX TO LISTS OF MEMBERS

The letters F, FE and FHM refer to the lists of Fellows, Fellows Emeriti and Foreign Honorary Members, respectively. The class and section are indicated by the numerals following.

Abbot, C. G. F, I:1
Abrams, L. R. F, II:2
Adams, C. A. F, I:4
Adams, C. F. F, III:4
Adams, C. R. F, I:1
Adams, R. F, I:3
Adrian, E. D. FHM, II:3
Aiken, H. H. F, I:1
Albright, F. F, II:4
Allee, W. C. F, II:3
Allen, A. W. F, II:4
Allis, W. P. F, I:2
d'Almada, J. FHM, III:2
Alonso, A. FHM, IV:3
Altamira y Crevea, R. FHM, IV:2
Alter, C. M. F, III:4
Amdur, I. F, I:3
Ames, A., Jr. F, I:2
Anderson, C. D. F, I:2
Anderson, E. F, II:2
Angell, J. W. F, III:3
Appleton, E. V. FHM, I:4
Archibald, R. C. F, I:1
Astwood, E. B. F, II:3
Aub, J. C. F, II:4
Aubert, M. FHM, IV:2
Ault, W. O. F, IV:2
Avery, O. T. F, II:4
Ayer, J. B. F, II:4
Bacon, L. F, IV:4
Bagge, G. A. FHM, III:4
Bailey, I. W. F, II:2
Bailey, L. H. F, II:2
Bainbridge, K. T. F, I:2
Baker, G. P. F, III:3
Baker, J. G. F, I:1
Balch, F. G. F, II:4
Balch, F. N. F, III:1
Baldensperger, F. FHM, IV:4
Baldwin, R. N. F, III:4
Ball, E. G. F, I:3
Bancroft, W. D. F, I:3
Banks, N. F, II:3
Bard, P. F, II:3
Barghoorn, E. S. F, II:2
Barnard, C. I. F, III:4
Barnes, J. L. F, I:1
Barnett, S. J. F, I:2
Barrows, H. K. F, I:4
Barth, K. FHM, IV:1
Bartlett, P. D. F, I:3
Batchelder, C. F. F, III:4
Bateman, A. M. F, II:1
Bauer, W. F, II:4
Baxter, G. P. F, I:3
Baxter, J. P., 3d. F, IV:2
Beams, J. W. F, I:2
Beatley, B. F, III:4
Beattie, J. A. F, I:3
Beebe-Center, J. G. F, IV:1
Beers, R. F. F, II:1
Belaunde, V. A. FHM, III:1
Belding, D. L. F, II:4
Bell, S. F, III:1
Benedict, F. G. F, II:3
Bennett, A. A. F, I:1
Benson, F. W. F, IV:4
Berg, P. FHM, III:1
Bergman, S. F, I:1
Berle, A. A., Jr. F, III:4
Bernstein, L. F, IV:4
Berry, C. H. F, I:4
Berry, G. P. F, II:4
Best, C. H. FHM, II:3
Bevis, H. L. F, III:2
Bigelow, H. B. F, II:3
Bigelow, R. P. F, II:3
Biggs, E. P. F, IV:4

Billings, M. P.	F, II:1
Bingham, B.	F, III:4
Birch, F.	F, I:2
Birkhoff, G.	F, I:1
Bissell, R. N., Jr.	F, III:3
Bitter, F.	F, I:2
Bixler, J. S.	F, IV:1
Black, S. B.	F, III:4
Blake, C. H.	F, II:3
Blake, F. G.	F, II:4
Blakeslee, A. F.	FE, II:2
Blanchard, A. A.	FE, I:3
Blanchard, R.	FHM, II:1
Blanshard, B.	F, IV:1
Blinks, L. R.	F, II:2
Bloch, B.	F, IV:3
Bloch, H.	F, IV:2
Blumer, G.	F, II:4
Blumgart, H. L.	F, II:4
Bock, A. V.	F, II:4
Bogert, M. T.	F, I:3
Bohr, N.	FHM, I:2
Bok, B. J.	F, I:1
Bonbright, J. C.	F, III:3
Bonner, C.	F, IV:3
Borgese, G. A.	FE, IV:3
Boring, E. G.	F, IV:1
Bosworth, W.	F, IV:4
Bovie, W. T.	F, II:3
Bowen, I. S.	F, I:1
Bowen, N. L.	F, II:1
Bowles, E. L.	F, I:4
Bowley, A. L.	FHM, III:3
Boyd, W. C.	F, I:3
Boyden, E. A.	F, II:3
Boysen-Jensen, P.	FHM, II:2
Brace, L. DeW.	F, III:4
Bradley, W. H.	F, II:1
Branch, C. R.	F, III:1
Breit, G.	F, I:2
Bremer, J. L.	F, II:3
Bridgman, P. W.	F, I:2
Briggs, L. J.	FE, I:2
Brigham, C. S.	F, IV:2
Brightman, E. S.	F, IV:1
Bronk, D. W.	F, III:4
Bronner, A. F.	F, III:3
Brooks, C. F.	F, II:1
Brooks, H.	F, I:2
Brooks, V. W.	F, IV:4
Brouwer, D.	F, I:1
Brown, D. V.	F, III:3
Brown, G. S.	F, I:4
Brown, J. N.	F, IV:4
Brown, T. H.	F, III:3
Bruce, H. A.	F, IV:1
Brüning, H.	FHM, III:4
Brues, C. T.	FE, II:3
Bucher, W. H.	F, II:1
Buck, C. D.	F, IV:3
Buckley, O. W.	F, III:4
Buddington, A. F.	F, II:1
Buerger, M. J.	F, II:1
Bunche, R. J.	F, III:2
Bundy, H. H.	F, III:2
Bunker, J. W. M.	F, II:3
Burchard, J. E.	F, III:4
Burdell, E. S.	F, III:4
Burgin, R.	F, IV:4
Burrows, M.	F, IV:1
Burwell, C. S.	F, II:4
Bush, J. N. D.	F, IV:4
Bush, V.	F, I:4
Butler, A. M.	F, II:4
Butterfield, V. L.	F, III:4
Cabot, G. L.	F, III:4
Cabot, T. D.	F, III:4
Cadbury, H. J.	F, IV:2
Cady, W. G.	F, I:2
Caldwell, R. G.	F, III:2
Cam, H. M.	F, IV:2
Campbell, C. G.	F, IV:2
Campbell, D. H.	F, II:2
Campbell, G. A.	F, I:2
Campbell, L. L.	F, I:2
Campbell, O. J.	F, IV:3
Canham, E. D.	F, III:4
Carmichael, L.	F, IV:1
Carnap, R.	F, IV:1
Carpenter, F. M.	F, II:1
Carpenter, T. M.	FE, II:3
Cartan, H.	FHM, I:1

Carré, J.-M. FHM, IV:4
Casagrande, A. F, I:4
Casey, R. P. F, IV:1
Castellani, Sir A. FHM, II:4
Castle, E. S. F, II:2
Castle, W. B. F, II:4
Castle, W. E. F, II:3
Castle, W. R., Jr. F, III:2
Caullery, M. FHM, II:3
Chafee, Z., Jr. F, III:1
Chaffee, E. L. F, I:2
Chamberlain, S. F, IV:4
Chamot, E. M. F, I:3
Chandrasekhar, S. F, I:1
Chao, Y. R. F, IV:3
Chase, G. H. FE, IV:2
Cheever, D. F, II:4
Chinard, G. FE, IV:2
Chipman, J. F, I:4
Churchill, E. D. F, II:4
Churchill, W. FHM, III:2
Claflin, W. H., Jr. F, III:4
Clark, G. N. FHM, IV:2
Clark, J. M. F, III:3
Clark, P. F. F, III:4
Clark, W. E. F, IV:3
Clark, W. I. F, II:4
Claudel, P. FHM, III:2
Clausen, J. C. F, II:2
Clay, H. FHM, III:3
Clayton, W. L. F, III:2
Cleland, R. E. F, II:2
Cleveland, L. R. F, II:3
Clifton, C. D. F, IV:4
Cochrane, E. L. F, I:4
Cockcroft, Sir J. D. FHM, I:2
Coffin, R. P. T. F, IV:4
Coffman, G. R. F, IV:3
Cogan, D. G. F, II:4
Cohen, M. F, I:4
Cohn, E. J. F, II:3
Cole, A. H. F, III:3
Cole, C. W. F, III:4
Cole, R. F, II:4
Cole, R. H. F, I:3
Collet, L. W. FHM, II:1
Collins, S. C. F, I:3
Compton, A. H. F, I:2
Compton, K. T. F, I:2
Comstock, A. L. F, III:4
Comstock, D. F. F, I:2
Conant, J. B. F, I:3
Conant, K. J. F, IV:4
Condon, E. U. F, I:2
Conklin, E. G. F, II:3
Constable, W. G. F, IV:4
Constance, L. F, II:2
Coolidge, C. A. F, III:1
Coolidge, E. S. F, IV:4
Coolidge, J. L. F, I:1
Coolidge, W. D. F, I:2
Coon, C. S. F, IV:2
Cooper, J. C. F, III:1
Cope, A. C. F, I:3
Cope, O. F, II:4
Copeland, C. T. FE, IV:4
Copeland, M. F, II:3
Copland, A. F, IV:4
Cori, C. F. F, I:3
Coryell, C. D. F, I:3
Cosío Villegas, D. FHM, III:3
Couch, W. T. F, III:4
Crane, R. S. F, IV:3
Crane, R. T. F, III:2
Crawford, F. H. F, I:2
Crew, H. F, I:2
Croce, B. FHM, IV:1
Cross, H. F, I:4
Cross, P. C. F, I:3
Crozier, W. J. F, II:3
Cunningham, W. J. FE, III:3
Curti, M. E. F, IV:2
Cutler, R. F, III:4
Dahl, O. G. C. F, I:4
Dale, Sir H. H. FHM, II:4
d'Almada, see Almada
Daly, R. A. FE, II:1
David, D. K. F, III:4
Davis, B. M. F, II:2
Davis, H. F, II:3
Davis, H. N. F, I:2
Davis, S. R. F, III:2

Davison, A. T. FE, IV:4
Davisson, C. J. F, I:2
Dawes, C. L. F, I:4
Dawson, A. B. F, II:3
Day, A. L. F, I:2
Dearborn, W. F. FE, IV:1
Debye, P. FHM, I:3
DeGolyer, E. L. F, II:1
De Hevesy, see Hevesy
de la Morandière, see Morandière
Del Vecchio, see Vecchio
de Margerie, see Margerie
Dempsey, E. W. F, II:3
Den Hartog, J. P. F, I:4
Denjoy, A. FHM, I:1
Dennison, H. S. F, III:4
Detwiler, S. R. F, II:3
DeVoto, B. A. F, IV:4
Dewey, B. F, I:4
Dewhurst, J. F. F, III:3
Dewing, A. S. F, III:3
Dexter, L. F, II:4
Dickey, J. S. F, III:2
Dickinson, J. F, III:1
Dickson, L. E. F, I:1
Dill, D. B. F, II:3
Dillon, T. H. F, I:4
Dinsmoor, W. B. F, IV:2
Dirac, P. A. M. FHM, I:2
Dodge, B. O. F, II:2
Dodge, R. G. F, III:1
Doering, C. R. F, III:3
Doisy, E. A. F, II:3
Dollard, C. F, III:4
Dollard, J. F, IV:2
Donham, W. B. F, III:3
Dos Passos, J. R. F, IV:4
Doty, P. M. F, I:3
Douglas, J. F, I:1
Douglas, P. H. F, III:2
Doyle, H. G. F, IV:3
Draper, C. S. F, I:4
Du Bois, E. F. F, II:4
Ducasse, C. J. F, IV:1
Dun, A. F, IV:1
Dunbar, C. O. F, II:1
Duncan, J. C. F, I:1
Dunn, G. F, I:4
Dunn, L. C. F, II:3
Durand, W. F. F, I:4
du Vigneaud, see Vigneaud
Dwiggins, W. A. F, IV:4
Eames, A. J. F, II:2
Edgell, G. H. F, IV:4
Edgerton, F. F, IV:3
Edsall, G. F, II:4
Edsall, J. T. F, I:3
Edwards, D. F. F, III:4
Einaudi, L. FHM, III:3
Einstein, A. FHM, I:2
Eliot, F. M. F, IV:1
Elisséeff, S. F, IV:3
Ell, C. S. F, III:4
Emerson, W. F, IV:4
Emmons, H. W. F, I:4
Enders, J. F. F, II:4
Esau, K. F, II:2
Esselen, G. J. F, I:3
Evans, A. W. F, II:2
Evans, G. C. F, I:1
Evans, H. M. F, II:3
Evans, L. H. F, IV:4
Evans, R. D. F, I:2
Evans, W. L. FE, I:3
Evatt, H. V. FHM, III:2
Ewell, A. W. F, I:2
Ewing, W. M. F, II:1
Fainsod, M. F, III:2
Fair, G. M. F, I:4
Fairchild, F. R. F, III:3
Farber, S. F, II:4
Farley, J. W. F, III:4
Farnsworth, H. E. F, I:2
Faulkner, J. M. F, II:4
Faull, J. H. FE, II:2
Fauré-Frémiet, E. FHM, II:3
Fay, S. B. FE, III:2
Fenn, W. O. F, II:3
Ferguson, W. S. FE, IV:2
Fergusson, S. P. FE, II:1
Field, W. L. W. F, III:4
Fieser, L. F. F, I:3

Finland, M. F, II:4
Fisher, R. A. FHM, I:1
Fisk, J. B. F, I:2
Fiske, C. H. F, II:3
Fitz, R. F, II:4
Flanders, R. E. F, III:4
Florin, R. FHM, II:2
Forbes, A. FE, II:3
Forbes, E. F, IV:4
Forbes, E. W. F, IV:4
Forbes, G. S. FE, I:3
Forbes, W. C. F, III:2
Ford, H. S. F, III:4
Ford, J. D. M. F, IV:3
Fosdick, R. B. F, III:4
Foster, A. S. F, II:2
Frame, J. E. F, IV:1
Franck, J. FHM, I:2
Frank, N. H. F, I:2
Frank, P. F, I:2
Franklin, P. F, I:1
Freeman, D. S. F, IV:2
Freeman, R. E. F, III:3
Friedrich, C. J. F, III:2
Frondel, C. F, II:1
Frost, R. F, IV:4
Fuess, C. M. F, IV:2
Fulbright, J. W. F, III:2
Fuller, L. L. F, III:1
Fulton, J. F. F, II:3
Furniss, E. S. F, III:2
Furry, W. H. F, I:2
Gamble, J. L. F, II:4
Gasser, H. S. F, II:3
Gast, P. R. F, II:2
Gaus, J. M. F, III:2
Geny, F. FHM, III:1
Gesell, A. L. F, II:3
Getting, I. A. F, I:4
Giauque, W. F. F, I:3
Gibb, H. A. R. FHM, IV:3
Gibson, R. F, II:1
Gilboy, G. F, I:4
Gilliland, E. R. F, I:4
Gilluly, J. F, II:1
Gilson, E. FHM, IV:1
Glueck, S. F, III:3
Golden, C. S. F, III:4
Goldman, H. F, IV:2
Goodale, J. L. F, II:4
Goodhart, A. L. FHM, III:1
Goodrich, C. F, III:3
Goodrich, H. F. F, III:1
Goodrich, L. M. F, III:2
Goodrich, W. F, IV:4
Gottschalk, L. F, IV:2
Graham, C. H. F, IV:1
Graton, L. C. F, II:1
Gray, F. C. F, III:4
Gray, L. H. F, IV:3
Green, T. F. F, III:1
Greene, Baron FHM, III:1
Greene, H. S. N. F, II:4
Greene, J. D. FE, III:4
Greene, W. C. F, IV:3
Greenewalt, C. H. F, III:4
Greep, R. O. F, II:3
Gregg, A. F, III:4
Gregory, H. E. F, II:1
Gregory, W. K. F, II:3
Grew, J. C. F, III:2
Grinnell, F. W. F, III:1
Griswold, A. W. F, III:4
Griswold, E. N. F, III:1
Gropius, W. F, IV:4
Gross, R. E. F, II:4
Gulick, C. B. F, IV:3
Gutenberg, B. F, II:1
Guthrie, E. R. F, IV:1
Hadamard, J. S. FHM, I:1
Haertlein, A. F, I:4
Haig, R. M. F, III:3
Hall, J. L. F, III:1
Hamilton, E. J. F, III:3
Hammond, M. F, IV:2
Hand, A. N. F, III:1
Hand, L. F, III:1
Hardy, A. C. F, I:2
Harington, Sir C. R. FHM, II:3
Harno, A. J. F, III:1
Harriman, W. A. F, III:4
Harris, L. F, I:3

Harris, S. E. F, III:3
Harrison, G. R. F, I:2
Harrison, R. G. F, II:4
Harvey, E. N. F, II:3
Haskins, C. P. F, III:3
Hastings, A. B. F, I:3
Hatch, W. H. P. FE, IV:1
Havens, R. D. F, IV:3
Hawkins, R. L. FE, IV:3
Hawthorne, W. R. F, I:4
Hawtrey, R. G. FHM, III:3
Hayes, B. H. F, IV:4
Hazen, H. L. F, I:4
Healy, W. F, IV:1
Hedges, J. B. F, IV:2
Hench, P. S. F, II:4
Hencken, H. O. F, IV:2
Hendrickson, G. L. F, IV:3
Herring, E. P. F, III:3
Herter, C. A. F, III:2
Hertig, A. T. F, II:4
Hertzsprung, E. FHM, I:1
Hevesy, G., De FHM, I:3
Hewett, D. F. F, II:1
Heyrovsky, J. FHM, I:3
Hill, A. V. FHM, II:3
Hill, B. H. F, IV:2
Hill, E. B. FE, IV:4
Hille, E. F, I:1
Hillyer, R. S. F, IV:4
Hindemith, P. FHM, IV:4
Hinshelwood, Sir C. N. FHM, I:3
Hippel, A. R., von F, I:4
Hisaw, F. L. F, II:3
Hitchcock, F. L. FE, I:1
Hoadley, L. F, II:3
Hoagland, H. F, II:3
Hockett, R. C. F, I:3
Hocking, W. E. FE, IV:1
Hodgkinson, H. D. F, III:4
Holborn, H. F, III:2
Holcombe, A. N. F, III:2
Holmes, A. FHM, II:1
Holmes, H. W. FE, III:3
Holmes, S. J. F, II:3
Hooker, S. B. F, II:4
Hooton, E. A. F, IV:2
Hopkins, E. M. F, III:4
Hopkinson, C. F, IV:4
Horsky, C. A. F, III:1
Horwood, M. P. F, I:4
Hoskins, H. L. F, IV:2
Hoskins, R. G. F, II:3
Hottel, H. C. F, I:4
Houssay, B. A. FHM, II:3
Houston, W. V. F, III:4
Howard, W. G. FE, IV:3
Howe, C. D. FHM, I:4
Howe, M. A. DeW. F, IV:4
Howe, M. DeW. F, III:1
Hu Shih. FHM, III:2
Hubbard, J. C. F, I:2
Hudnut, J. F, IV:4
Huggins, C. B. F, II:4
Hull, C. L. F, IV:1
Hull, G. F. F, I:2
Hume, E. E. F, II:4
Hunsaker, J. C. F, I:4
Hunt, B. C. F, III:3
Hunt, F. V. F, I:2
Huntington, A. M. F, IV:4
Huntington, E. V. F, I:1
Huntress, E. H. F, I:3
Huntsman, A. G. FHM, II:3
Hurewicz, W. F, I:1
Hurlbut, C. S., Jr. F, II:1
Hutchinson, G. E. F, II:3
Ippen, A. T. F, I:4
Iselin, C. O. F, II:1
Ivins, W. M., Jr. F, IV:4
Jack, J. R. F, I:4
Jackson, H., Jr. F, II:4
Jackson, W. A. F, IV:4
Jaeger, W. W. F, IV:3
Jaggar, T. A. F, II:1
Jakobson, R. F, IV:3
Janeway, C. A. F, II:4
Jellinek, E. M. F, I:1
Jessup, P. C. F, III:2
Joffé, A. F. FHM, I:2
Johnson, L. J. F, I:4
Johnson, M. M. F, III:1

Johnston, I. M. F, II:2
Jones, D. F. F, II:2
Jones, H. M. F, IV:4
Jones, Sir H. S. FHM, I:1
Jordan, W. K. F, IV:2
Joslin, E. P. F, II:4
Kármán, T., von F, I:4
Karpovich, M. F, IV:2
Keefer, C. S. F, II:4
Keenan, J. H. F, I:4
Keith, Sir A. FHM, II:4
Keller, C. T. F, III:4
Kelsen, H. FHM, III:1
Kemble, E. C. F, I:2
Kendall, E. C. F, II:3
Kendall, H. P. F, III:4
Keniston, H. F, IV:3
Kent, R. G. F, IV:3
Ketchum, P. F, III:1
Keyes, F. G. F, I:3
Killian, J. R., Jr. F, III:4
Kinkeldey, O. F, IV:4
King, R. W. P. F, I:2
Kirkwood, J. G. F, I:3
Kistiakowsky, G. B. F, I:3
Klausner, J. FHM, IV:3
Kleinschmidt, R. V. F, I:4
Kluckhohn, C. K. M. F, IV:2
Knight, F. H. F, III:3
Knopf, A. F, II:1
Knudson, A. C. F, IV:1
Köhler, W. FHM, IV:1
Kraus, C. A. F, I:3
Krayer, O. F, II:3
Kretschmer, P. FHM, IV:3
Kroeber, A. L. F, IV:2
Kuiper, G. P. F, I:1
Kurath, H. F, IV:3
LaFarge, J. F, IV:1
Lamb, A. B. F, I:3
Lancaster, H. C. F, IV:3
Land, E. H. F, I:2
Landis, E. M. F, II:3
Landis, J. M. F, III:1
Langley, E. F. FE, IV:3
Langmuir, I. F, I:3
Lansing, A. F, IV:2
La Piana, G. FE, IV:2
Lapicque, L. E. FHM, II:3
Larsen, E. S., Jr. F, II:1
Lashley, K. S. F, IV:1
Latourette, K. S. F, IV:2
Laue, M. F. T., von FHM, I:2
Lawrence, E. O. F, I:2
Lawrence, R. R. FE, I:4
Lawrence, W. H. F, I:4
Lawson, A. C. F, II:1
Lazarsfeld, P. F. F, III:3
LeCorbeiller, P. E. F, I:2
Lee, R. I. F, II:4
Leeds, M. E. F, III:4
Leith, C. K. F, II:1
Leland, W. G. F, IV:2
Lessells, J. M. F, I:4
Levin, H. T. F, IV:3
Levine, J. F, IV:4
Levine, S. A. F, II:4
Levinson, N. F, I:1
Levorsen, A. I. F, II:1
Lewis, C. I. F, IV:1
Lewis, W. K. F, I:3
Lie, T. H. FHM, III:2
Lilienthal, D. E. F, III:4
Lillie, R. S. F, II:3
Lin, Chia-Chiao F, I:1
Lindblad, B. FHM, I:1
Linderström-Lang, K. U. FHM, II:3
Lindsay, R. B. F, I:2
Linforth, I. M. F, IV:3
Lingane, J. J. F, I:3
Lingelbach, W. E. F, IV:2
Linton, R. F, IV:2
Lipmann, F. A. F, I:3
Lippmann, W. F, III:2
Little, C. C. F, III:4
Locke, E. A. F, II:4
Loeb, R. F. F, II:4
Lombardi, L. FHM, I:4
Long, C. N. H. F, II:3
Longcope, W. T. F, II:4
Lord, M. E. F, IV:4

Lord, R. C. F, I:3
Lowe, E. A. F, IV:3
Lowell, R. F, III:4
Lull, R. S. F, II:3
Lundegardh, H. G. FHM, II:2
Lutz, B. R. F, II:3
Luyten, W. J. F, I:1
Lyman, T. F, I:2
Lyons, L. M. F, III:4
McAdams, W. H. F, I:4
McEwen, G. F. F, II:1
MacGregor, C. W. F, I:4
McIlwain, C. H. FE, IV:2
MacInnes, D. A. F, I:3
MacIver, R. M. F, III:3
McKeehan, L. W. F, I:2
McKeon, R. P. F, IV:1
McKinstry, H. E. F, II:1
MacLane, S. F, I:1
McLaren, W. W. F, III:3
McLaughlin, D. H. F, II:1
Maclaurin, W. R. F, III:3
MacLeish, A. F, IV:4
Macmillan, Baron FHM, III:1
McNair, M. P. F, III:3
Macneil, S. F, III:1
MacNider, W. de B. F, II:4
Maginnis, C. D. F, IV:4
Magruder, C. F, III:1
Mahony, T. H. F, III:1
Maheshwari, P. FHM, II:2
Makarewicz, J. FHM, III:1
Malamud, W. F, II:4
Malik, C. H. FHM, III:2
Malone, D. F, III:4
Mangelsdorf, P. C. F, II:2
Mann, T. F, IV:4
Manship, P. F, IV:4
Margerie, E., de FHM, II:1
Marin, J. F, IV:4
Mark, K. L. FE, I:3
Marks, L. S. F, I:4
Marsh, D. L. F, III:4
Marshall, L. C. F, III:3
Martin, W. T. F, I:1
Mason, D. G. F, IV:4
Mather, F. J. F, IV:4
Mather, K. F. F, II:1
May, M. A. F, III:3
Mazon, P. FHM, IV:3
Mead, M. F, IV:2
Mead, W. J. F, II:1
Means, J. H. F, II:4
Medina, H. F, III:1
Mees, C. E. K. F, I:3
Meinecke, F. FHM, IV:2
Melander, A. L. F, II:3
Menéndez Pidal, R. FHM, IV:3
Menzel, D. H. F, I:1
Meriam, R. S. F, III:3
Meritt, B. D. F, IV:3
Merriam, C. E. F, III:2
Merrill, E. D. FE, II:2
Merton, R. K. F, III:3
Metcalf, K. DeW. F, III:4
Metz, C. W. F, II:3
Meyer, A. FHM, I:4
Meyer, K. F. F, II:3
Milas, N. A. F, I:3
Miller, G. S. F, II:3
Miller, W. J. F, II:1
Millikan, R. A. F, I:2
Mills, F. C. F, III:3
Mimno, H. R. F, I:2
Miner, L. M. S. F, II:4
Mises, R., von F, I:4
Mitchell, S. F, IV:2
Mitchell, S. A. F, I:1
Mitchell, W. DeW. F, III:1
Moe, H. A. F, III:4
Monroe, A. E. F, III:3
Moore, C. R. F, II:3
Morandière, L. J., de la FHM, III:1
Morey, C. R. F, IV:4
Morgan, E. M. F, III:1
Morison, S. E. F, IV:2
Morris, F. K. F, II:1
Morse, M. F, I:1
Morse, P. M. F, I:2
Morton, A. A. F, I:3
Moss, W. L. F, II:4
Moulton, F. R. F, I:1

Mueller, E. F, I:3
Mueller, H. F, I:2
Mueller, J. H. F, II:4
Muller, H. J. F, II:3
Mumford, L. F, IV:4
Munch, C. F, IV:4
Munro, T. F, IV:4
Munro, W. B. F, III:2
Murdock, G. P. F, IV:2
Murdock, K. B. F, IV:4
Murphy, A. E. F, IV:1
Murphy, G. F, IV:1
Murray, G. FHM, IV:4
Murray, H. A., Jr. F, IV:1
Mynors, R. A. B. FHM, IV:3
Nash, N. B. F, IV:1
Nathanson, I. T. F, II:4
Navarro, T. FHM, IV:3
Neale, J. E. FHM, IV:2
Nehru, J. FHM, III:2
Neugebauer, O. E. F, IV:2
Neumann, J., von F, I:1
Nevins, A. F, IV:2
Newhouse, W. H. F, II:1
Nicholas, J. S. F, II:3
Niemeyer, O. FHM, IV:4
Nilsson, M. P. FHM, IV:2
Nitze, W. A. F, IV:3
Nock, A. D. F, IV:1
Nolan, T. B. F, II:1
Northrop, F. S. C. F, IV:1
Northrop, J. H. F, I:3
Norton, J. T. F, I:4
Nourse, E. G. F, III:3
Noyes, G. R. F, IV:3
Noyes, W. A., Jr. F, I:3
O'Brian, J. L. F, III:1
O'Connor, J. F, IV:1
Oertel, H. FHM, IV:3
Ogburn, W. F. F, III:3
Oldenberg, O. F, I:2
Olmsted, F. L. F, I:4
Oncley, J. L. F, I:3
Onsager, L. F, I:3
Oort, J. H. FHM, I:1
Oppenheimer, J. R. F, I:2
Orowan, E. F, I:4
Osgood, R. B. FE, II:4
Osterhout, W. J. V. F, II:2
Page, L. F, I:2
Palache, C. FE, II:1
Palacios, A. L. FHM, III:4
Paneth, F. FHM, I:3
Panofsky, E. F, IV:4
Park, C. E. F, IV:1
Parker, G. H. F, II:3
Parkman, H. F, III:1
Parodi, L. R. FHM, II:2
Parsons, T. F, III:3
Patch, H. R. F, IV:3
Paton, W. A. F, III:3
Paul, J. R. F, II:4
Pauli, W. FHM, I:2
Pauling, L. C. F, I:3
Payne-Gaposchkin, C. F, I:1
Pearse, L. F, I:4
Pease, A. S. FE, IV:3
Peers, E. A. FHM, IV:4
Peirce, G. J. F, II:2
Pender, H. F, I:4
Penfield, W. G. FHM, II:4
Pepper, G. W. F, III:1
Peters, J. L. F, II:3
Peters, R. A. FHM, II:3
Pfeiffer, R. H. F, IV:2
Phillips, H. B. F, I:1
Phillips, W. F, III:2
Philpott, A. J. F, IV:4
Pickard, G. W. F, I:4
Pierce, G. W. F, I:2
Piéron, H. FHM, IV:1
Pigou, A. C. FHM, III:3
Pilsbry, H. A. F, II:3
Pincus, G. F, II:3
Piston, W. H. F, IV:4
Plough, H. H. F, II:3
Porsild, A. E. FHM, II:2
Post, C. R. FE, IV:4
Pound, R. F, III:1
Prager, W. F, I:4
Prandtl, L. FHM, I:4
Pratt, C. C. F, IV:1

Pratt, F. H.	F, II:3
Pratt, J. H.	F, II:4
Prescott, S. C.	FE, I:3
Price, L.	F, IV:4
Probst, E.	FHM, I:4
Proger, S.	F, II:4
Purcell, E. M.	F, I:2
Putnam, H.	F, III:4
Putnam, T. J.	F, II:4
Qua, S. E.	F, III:1
Quinby, W. C.	F, II:4
Quine, W. V. O.	F, IV:1
Rackemann, F. M.	F, II:4
Ramsey, N. F.	F, I:2
Rand, H. W.	F, II:3
Rand, W. M.	F, III:4
Raper, K. B.	F, II:2
Rapport, D.	F, II:3
Raup, H. M.	F, II:2
Raymond, P. E.	FE, II:1
Read, C.	F, IV:2
Redfield, A. C.	F, II:3
Redfield, R.	F, IV:2
Reichenbach, H.	F, IV:1
Reid, H. R.	F, III:4
Reissner, E.	F, I:1
Renner, O.	FHM, II:2
Richards, A. N.	F, II:3
Riddle, O.	F, II:3
Rist, C.	FHM, III:3
Robbins, W. J.	F, II:2
Robertson, D. H.	FHM, III:3
Robinson, D. M.	F, IV:2
Robinson, F. N.	F, IV:3
Robinson, Sir R.	FHM, I:3
Rochow, E. G.	F, I:3
Rock, J.	F, II:4
Roeder, K. D.	F, II:3
Roethlisberger, F. J.	F, III:3
Rogers, A. F.	F, II:1
Rollins, H. E.	F, IV:3
Rollins, R. C.	F, II:2
Romer, A. S.	F, II:3
Romero, F.	FHM, IV:1
Ross, Sir W. D.	FHM, IV:1
Rossby, C.-G. A.	F, II:1
Rossi, B. B.	F, I:2
Rostovtzeff, M. I.	F, IV:2
Rotch, A. G.	F, III:4
Rowntree, B. S.	FHM, III:4
Rüdenberg, R.	F, I:4
Ruggles, A. H.	F, II:4
Ruggles, C. O.	F, III:3
Russell, F. F.	FE, II:4
Russell, G. E.	FE, I:4
Russell, H. N.	F, I:1
Ruthven, A. G.	F, II:3
Ruzicka, L.	FHM, I:3
Sachar, A. L.	F, III:4
Sachs, P. J.	F, IV:4
Saha, M. N.	FHM, I:1
Salter, W. T.	F, II:4
Salvemini, G.	F, III:2
Samuelson, P. A.	F, III:3
Sanders, H. A.	F, IV:3
Sanders, T. H.	F, III:3
Sarton, G.	FE, IV:2
Sawyer, C. H.	F, IV:4
Sayre, F. B.	F, III:1
Scatchard, G.	F, I:3
Schaller, W. T.	F, II:1
Schell, E. H.	F, III:4
Schmidt, K. P.	F, II:3
Schmitt, B. E.	F, IV:2
Schmitt, F. O.	F, II:3
Schneider, H. W.	F, IV:1
Scholte, J. H.	FHM, IV:4
Schrödinger, E.	FHM, I:2
Schücking, L. L.	FHM, IV:4
Schumb, W. C.	F, I:3
Schwinger, J. S.	F, I:2
Scott, A. W.	F, III:1
Scott, D.	F, IV:2
Sedgwick, E.	F, IV:4
Sedgwick, H. D.	F, IV:4
Selekman, B. M.	F, III:3
Seymour, C.	F, III:4
Seznec, J. J.	F, IV:3
Shajn, G. A.	FHM, I:1
Shapley, H.	F, I:1
Shattuck, G. C.	F, II:4
Shattuck, H. L.	F, III:4

Shaw, H. S. F, III:4
Sheehan, J. C. F, I:3
Sherburn, G. W. F, IV:3
Sherrill, H. K. F, IV:1
Sherrill, M. S. F, I:3
Sherrington, Sir C. S. FHM, II:4
Sherwood, T. K. F, I:4
Shimer, H. W. FE, II:1
Shryock, R. H. F, IV:2
Shuster, G. N. F, III:4
Sibelius, J. J. C. FHM, IV:4
Sidgwick, N. V. FHM, I:3
Sigerist, H. E. FHM, IV:2
Simmons, J. S. F, II:4
Simonds, B. F, IV:4
Simpson, G. G. F, II:1
Singleton, C. S. F, IV:3
Sinnott, E. W. F, II:2
Slater, J. C. F, I:2
Slipher, V. M. F, I:1
Sloan, A. P., Jr. F, III:4
Sloan, G. A. F, III:4
Smith, A. C. F, II:2
Smith, C. S. F, I:4
Smith, G. M. F, II:2
Smith, H. W. F, II:3
Smith, L. B. F, I:3
Smith, P. F, III:4
Smith, R. M. F, II:4
Smith, Sir W. W. FHM, II:2
Snyder, J. C. F, II:4
Soderberg, R. C. F, I:4
Sollmann, T. H. F, II:4
Sonneborn, T. M. F, II:3
Sorokin, P. A. F, III:3
Sosman, M. C. F, II:4
Spaak, P.-H. FHM, III:2
Spalding, W. R. FE, IV:4
Spaulding, H. N. F, III:4
Sperry, W. L. F, IV:1
Spinden, H. J. F, IV:2
Spoehr, H. A. F, II:2
Spofford, C. M. F, I:4
Sprague, O. M. W. F, III:3
Stace, W. T. F, IV:1
Stadler, L. J. F, II:2
Stafford, R. H. F, IV:1
Stakman, E. C. F, II:2
Stanley, W. M. F, I:3
Starck, T. F, IV:3
Stearns, A. W. F, III:4
Stebbins, J. F, I:1
Stephenson, C. C. F, I:3
Stetson, H. C. F, II:1
Stetson, H. T. F, I:1
Stevens, S. S. F, IV:1
Stewart, G. W. F, I:2
Stewart, R. B. F, III:2
Stockbarger, D. C. F, I:2
Stockmayer, W. H. F, I:3
Stoke, H. W. F, III:3
Stouffer, S. A. F, III:3
Stratton, J. A. F, I:2
Stravinsky, I. FHM, IV:4
Street, J. C. F, I:2
Struik, D. J. F, I:1
Struve, O. F, I:1
Sturtevant, A. H. F, II:3
Sulzberger, A. H. F, III:4
Sumner, J. B. F, I:3
Svedberg, T. FHM, I:3
Sverdrup, H. U. F, II:1
Swan, T. W. F, III:1
Taft, P. F, III:3
Takayanagi, K. FHM, III:2
Talbot, F. B. FE, II:4
Talbot, N. B. F, II:4
Tarski, A. F, IV:1
Taylor, A. F, IV:3
Taylor, C. F. F, I:4
Taylor, C. H. F, IV:2
Taylor, C. L., Jr. F, IV:1
Taylor, E. S. F, I:4
Taylor, F. H. F, IV:4
Taylor, H. S. F, I:3
Taylor, L. R. F, IV:2
Taylor, W. R. F, II:2
Terman, L. M. F, IV:1
Terzaghi, K. F, I:4
Thannhauser, S. J. F, II:4
Theorell, H. FHM, II:3
Thimann, K. V. F, II:2

Thomas, F. W. FHM, IV:3
Thompson, M. deK. FE, I:2
Thompson, R. F, IV:4
Thomson, G. H. FHM, IV:1
Thomson, Sir G. P. FHM, I:2
Thomson, W. F, IV:3
Thorn, G. W. F, II:4
Thurstone, L. L. F, IV:1
Tillich, P. J. F, IV:1
Tisza, L. F, I:2
Tolman, E. C. F, IV:1
Torrey, C. C. F, IV:3
Tosdal, H. R. F, III:3
Toynbee, A. J. FHM, IV:2
Tozzer, A. M. F, IV:2
Trevelyan, G. M. FHM, IV:2
Trump, J. G. F, I:4
Tsien, H.-S. F, I:4
Tucker, D. S. F, III:3
Turner, C. E. FE, III:4
Tuve, M. A. F, I:2
Twaddell, W. F. F, IV:3
Tyzzer, E. E. F, II:4
Ulich, R. F, III:3
Ullman, B. L. F, IV:3
Unwin, Sir S. FHM, IV:1
Urey, H. C. F, I:3
Usher, A. P. FE, III:3
Vallarta, M. S. F, I:2
Vallée Poussin, C. J., de la FHM, I:1
Van de Graaff, R. J. F, I:2
Van Hook, L. R. FE, IV:3
Van Niel, C. B. F, II:2
Van Vleck, J. H. F, I:2
Vaughan, T. W. F, II:1
Veblen, O. F, I:1
Vecchio, G., Del FHM, III:1
Verhoeff, F. H. F, II:4
Vickery, H. B. F, II:3
Viets, H. R. F, IV:2
Vigneaud, V., du F, I:3
Viner, J. F, III:3
Voegelin, C. F. F, IV:3
von Hippel, see Hippel
von Kármán, see Kármán
von Laue, see Laue
von Mises, see Mises
von Neumann, see Neumann
Wagner, K. W. FHM, I:4
Wagner, M. F, IV:4
Waksman, S. A. F, II:2
Wald, G. F, II:3
Walsh, J. L. F, I:1
Wambaugh, S. F, III:2
Wardlaw, C. W. FHM, II:2
Warner, E. P. F, I:4
Warren, B. E. F, I:2
Warren, R. P. F, IV:4
Warren, S. F, II:4
Washburn, H. B. FE, IV:1
Watson, J. B. F, IV:1
Wearn, J. T. F, II:4
Weber, H. C. F, I:4
Webster, D. L. F, I:2
Webster, W. F, III:4
Weeks, E. A., Jr. F, IV:4
Weisskopf, V. F. F, I:2
Weizmann, C. FHM, I:3
Welles, S. F, III:2
Welsh, J. H. F, II:3
Went, F. W. F, II:2
West, R. FHM, IV:4
Weston, G. B. FE, IV:3
Weston, K. E. F, IV:4
Weston, W. H., Jr. F, II:2
Wetmore, R. H. F, II:2
Weyl, H. FHM, I:1
Weysse, A. W. F, II:3
Whicher, G. F. F, IV:4
Whipple, F. L. F, I:1
White, L. D. F, III:4
White, P. D. F, II:4
White, P. R. F, II:2
White, W. L. F, II:2
Whitehead, T. N. F, III:3
Whitehill, W. M. F, III:4
Whitman, E. A. FE, III:1
Whitman, W. G. F, I:4
Whitney, W. R. F, I:3
Whittlesey, D. S. F, II:1
Widder, D. V. F, I:1
Wieland, H. FHM, I:3

Wigner, E. P. F, I:2
Wilbur, J. B. F, I:4
Wilkins, E. H F, IV:3
Williams, C. M. F, II:3
Williams, J. H. F, III:3
Williams, R. S. FE, I:3
Willits, J. H. F, III:3
Willoughby, W. F. F, III:2
Wilson, C. E. F, III:4
Wilson, C. L. F, III:4
Wilson, E. B. F, I:2
Wilson, E. B., Jr. F, I:3
Wilson, H. E. F, III:2
Wilson, J. W. F, II:3
Wilson, T. J. F, III:4
Wind, E. F, IV:4
Winship, L. L. F, III:4
Wislocki, G. B. F, II:3
Wolbach, S. B. F, II:4
Wolfrom, M. L. F, I:3
Wolfson, H. A. F, IV:3
Wolman, L. F, III:3
Wong Wen-hao FHM, II:1
Wood, R. W. F, I:2
Woodward, R. B. F, I:3
Woodworth, R. S. F, IV:1
Worrell, W. H. FE, IV:3
Wright, B. F. F, III:4
Wright, Baron FHM, III:1
Wright, C. H. C. FE, IV:4
Wright, F. E. F, II:1
Wright, L. B. F, IV:3
Wright, Q. F, III:2
Wright, S. F, II:3
Wriston, H. M. F, III:4
Wu, J. C. H. FHM, III:1
Wulff, J. F, I:4
Wurster, W. W. F, IV:4
Wyckoff, R. W. G. F, II:3
Wyman, J., Jr. F, II:3
Wyman, L. C. F, II:3
Wyzanski, C. E., Jr. F, III:1
Yeomans, H. A. F, III:2
Yerkes, R. M. F, II:3
Young, B. L. F, III:4
Young, O. D. F, III:4
Young, R. C. F, I:3
Zacharias, J. R. F, I:2
Zariski, O. F, I:1
Zimmerman, C. C. F, III:3
Zimmern, Sir A. FHM, III:2
Zworykin, V. K. F, I:4

STATUTES

THE AMERICAN ACADEMY OF ARTS AND SCIENCES

Adopted November 8, 1911: *amended* May 8, 1912, *January* 8, *and May* 14, 1913, *April* 14, 1915, *April* 12, 1916, *April* 10, 1918, *May* 14, 1919, *February* 8, *April* 12, *and December* 13, 1922, *February* 14, *March* 14, *and October* 10, 1923, *March* 10, 1926, *May* 9, 1928, *April* 8, *and November* 11, 1931, *April* 12, 1933, *February* 14, 1934, *December* 14, 1938, *January* 11, *April* 12, 1939, *May* 8, 1940, *May* 14, 1941, *November* 18, 1942, *and January* 12, 1944, *May* 9, 1945, *November* 14, 1945, *February* 2, 1946, *October* 9, 1946, *October* 8, 1947, *March* 9, 1949, *and October* 11, 1950.

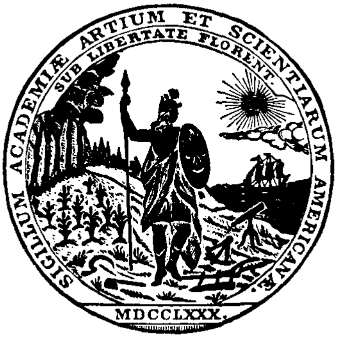
CHAPTER I

THE ACADEMY AND ITS CORPORATE SEAL

ARTICLE 1. The American Academy of Arts and Sciences is a body politic and corporate by the same name, forever, established by the Council and House of Representatives in General Court of the Province of Massachusetts Bay as recorded in Chap. 46 of the Acts of 1779.

ARTICLE 2. As enacted above, the stated end and design of the institution of the said Academy is to promote and encourage the knowledge of the antiquities of America, and of the natural history of the country, and to determine the uses to which the various natural productions of the country may be applied; to promote and encourage medical discoveries, mathematical disquisitions, philosophical enquiries and experiments, astronomical, meteorological and geographical observations; and improvements in agriculture, arts, manufacture and commerce; and, in fine, to cultivate every art and science which may tend to advance the interest, honor, dignity and happiness of a free, independent and virtuous people.

ARTICLE 3. The Corporate Seal of the Academy shall be as here depicted.


SIGILLUM ACADEMIÆ ARTIUM ET SCIENTIARUM AMERICANÆ.
SUB LIBERTATE FLORENT.
MDCCLXXX.

ARTICLE 4. The Secretary shall have the custody of the Corporate Seal.

See Chap. v, art. 3; chap. vi, art. 1.

CHAPTER II

MEMBERSHIP AND DUES

ARTICLE 1. The Academy shall consist of Fellows, elected from the citizens or residents of the United States of America, Fellows Emeriti, and Foreign Honorary Members. They are arranged in four Classes, according to the Arts and Sciences in which they are severally proficient, and each Class shall be divided into four Sections, namely:

CLASS I. *The Mathematical and Physical Sciences*
- Section 1. Mathematics and Astronomy
- Section 2. Physics
- Section 3. Chemistry
- Section 4. Technology and Engineering

CLASS II. *The Natural and Physiological Sciences*
- Section 1. Geology, Mineralogy, and Physics of the Globe
- Section 2. Botany
- Section 3. Zoology and Physiology
- Section 4. Medicine and Surgery

CLASS III. *The Social Arts*
- Section 1. Jurisprudence
- Section 2. Government, International Law, and Diplomacy
- Section 3. Economics and Sociology
- Section 4. Administration and Affairs

CLASS IV. *The Humanities*
- Section 1. Theology, Philosophy, and Psychology
- Section 2. History, Archaeology, and Anthropology
- Section 3. Philology
- Section 4. The Fine Arts and Belles Lettres

ARTICLE 2. The number of Fellows shall not exceed one thousand, of whom not more than two-thirds shall be residents of New England, nor shall there be more than two hundred and seventy-five in any one Class.

Any Fellow of the Academy on retiring from his academic or other regular duties may, if he so requests in writing, and with the approval of the Council, be transferred to the status of Fellow Emeritus.

Fellows Emeriti shall be exempt from payment of dues. They may not hold elective office in the Academy, nor serve on Standing Committees, nor vote at meetings, but shall have all the other privileges of Fellowship.

Fellows Emeriti shall be separately classified and shall be outside the statutory limit set on the total number of Fellows and the number in a given Class.

See Chap. ix, art. 3; chap. x, art. 1.

Article 3. The number of Foreign Honorary Members shall not exceed one hundred and fifty. They shall be chosen from citizens of foreign countries who are eminent for their discoveries or attainments in any of the Classes above enumerated. There shall be not more than forty-five in any one Class.

Article 4. Diplomas signed by the President and the Vice-President of the Class to which the member belongs, and countersigned by the Secretary, shall be given to Fellows and Foreign Honorary Members.

Article 5. If any person, after being notified of his election as Fellow or Foreign Honorary Member, shall neglect for six months to accept in writing, his election shall be void.

Article 6. Every Fellow hereafter elected shall pay an Admission Fee of ten dollars: if he shall neglect to pay this Fee within six months of the date of his election, his election shall become void.

Every Fellow resident within fifty miles of Boston shall pay, unless he enjoys statutory exemption, such Annual Dues, not exceeding fifteen dollars, as shall be voted annually in March by the Academy. These shall become due on the first of June.

Every Fellow residing more than fifty miles from Boston elected after 1938 shall pay, unless he enjoys statutory exemption, and other non-resident Fellows may pay, Annual Dues equal to one-half the amount set for resident Fellows.

Article 7. Exemption from further payment of Annual Dues shall be granted by the Council or its designate to any Fellow who has paid such dues, at either resident or non-resident rate, for forty years; or having attained the age of seventy-five, has paid dues for twenty-five years.

Exemption from further payment of Annual Dues shall be granted forthwith by the Council or its designate to any Fellow who pays into the treasury of the Academy the sum of two hundred dollars for the purpose of commuting subsequent dues, in addition to his previous payments.

Article 8. Any Fellow, resident or non-resident, who shall neglect to pay his Annual Dues for six months after they are due and who ignores notification by the Treasurer of the requirements of this Article of the Statutes shall cease to be a Fellow.

Article 9. Upon petition of any Fellow, the Council may by a majority vote suspend the application of any penalties hereinabove prescribed in this chapter for an additional period of time not longer than three months.

Article 10. Only Fellows who pay Annual Dues or are exempted from further payment by commutation of dues or otherwise as set forth in Article 7 of this Chapter may hold elective office in the Academy or serve on Standing Committees as specified in Chapter XI, or vote at meetings.

Article 11. If, in the opinion of a majority of the entire Council, any Fellow or Foreign Honorary Member shall have rendered himself unworthy of a place in the Academy, the Council shall recommend to the Academy the termination of his membership; and if three-fourths of the Fellows present out of a total attendance of not less than fifty at a Stated Meeting, or at a Special Meeting called for the purpose, shall adopt this recommendation, his name shall be stricken from the Roll.

See Chap. iii; chap. vi, art. 5 and 6; chap. x, art. 1.

CHAPTER III

Nomination and Election of Fellows and Foreign Honorary Members

The procedure for nomination and election of Fellows and Foreign Honorary Members shall be as follows:

Article 1. Nominations may be made at any time by any two Fellows in writing on forms to be provided by the Secretary and shall be referred by him to the Committee on Membership.

The Committee on Membership shall meet following the stated meetings of the Academy in May, November, February and March, and at such other times as it may determine, to appraise nominations received by it from the Fellows from time to time, to originate further nominations, and to approve as candidates for election those receiving the favorable vote of two-thirds of the committee members present in any meeting attended by not less than five of its members.

Immediately following its meeting in February, the Committee shall cause to be sent to every Fellow a list of nominees, with biographical and professional data thereon, together with names of nominators, for appraisal, expression of preference, or other comment by the Fellows.

The Committee, at its March meeting, shall review all nominations, together with comments by the Fellows thereon, and shall compile a list of approved candidates for the annual election of Fellows and Foreign Honorary Members. It shall present this list together with data pertaining thereto to the Council not later than at the Stated Meeting of the Council in April.

The Council, by vote of the majority of members present at a meeting, shall make final nominations from the list of approved persons recommended by the Committee on Membership for election by the Fellows.

ARTICLE 2. Election of Fellows and Foreign Honorary Members shall be made by a majority of the Fellows present at the Annual Meeting in May, from the nominations presented at that meeting by the Council.

ARTICLE 3. Each Fellow-elect or Foreign Honorary Member-elect shall immediately following his election be notified thereof in writing by the Secretary.

See Chap. ii; chap. vi, art. 5; chap. x, art. 1; chap. xi, art. 1 (ii).

CHAPTER IV

OFFICERS

ARTICLE 1. The Officers of the Academy shall be a President (who shall be Chairman of the Council), four Vice-Presidents (one from each Class), a Secretary (who shall be Secretary of the Council), a Treasurer, a Librarian, and an Editor, all of whom shall be elected by ballot at the Annual Meeting, and shall take office at the close of that meeting, and shall hold their respective offices for one year, and until others are duly chosen and take office.

There shall be also sixteen Councillors, one from each Section of each Class. At each Annual Meeting four Councillors, one from each Class, shall be elected by ballot to serve for a term of four years, and they shall take office at the close of that meeting, and shall hold office until others are duly chosen and take office. The same Fellows shall not be eligible for two successive terms.

The Councillors, with the officers previously named, and the Chairmen of the Standing Committees, *ex officiis*, shall constitute the Council.

See Chap. xi, art. 1.

Article 2. If any officer be unable, through death, absence, or disability, to fulfill the duties of his office, or if he shall resign, his place may be filled by the Council in its discretion for any part or the whole of the unexpired term.

Article 3. The Nominating Committee, as constituted under Chapter XI, Article 1 (xi), shall prepare a list of nominees for the several offices to be filled, and for the Standing Committees, and file it with the Secretary not later than eight weeks before the Annual Meeting.

The Secretary shall transmit to the Fellows, previous to the stated meeting in April, the report of the Nominating Committee.

Article 4. Independent nominations for any office, if signed by at least twenty Fellows having the right to vote, and received by the Secretary not less than twenty days before the Annual Meeting, shall be included in the election procedure.

Article 5. The Secretary shall prepare for use in voting at the Annual Meeting a ballot containing the names of all persons duly nominated for office.

CHAPTER V

The President

Article 1. The President, or in his absence a Vice-President, shall preside at meetings of the Academy.

See Chap. vi, art. 3.

Article 2. The President shall be the chief executive officer of the Academy. He shall present to the Council for its consideration all matters pertinent to the interests of the Academy and to the discharge of its obligations to the community or to the advancement of scholarship.

Article 3. Any deed or writing to which the Corporate Seal is to be affixed, except leases of real estate, shall be executed in the name of the Academy by the President or in the event of his death, absence, or inability, by one of the Vice-Presidents, when thereto duly authorized by the Council.

Article 4. In case of incapacity of the President, the Council shall designate a Vice-President to carry out the duties of the office.

See Chap. ii, art. 4; chap. iv, art. 1, 3; chap. vi, art. 3; chap. viii, art. 4; chap. x, art. 3; chap. xi, art. 1 (ii), (iii), (x); chap. xii, art. 1.

CHAPTER VI

The Secretary

Article 1. The Secretary shall provide for the custody of the Charter, Corporate Seal, Statute Book, Journals of the Academy, and other Archives.

Article 2. He shall be responsible for the correspondence of the Academy and of the Council. At each meeting of the Council he shall present any important communications addressed to the Academy which have been received since the previous meeting, and at the next meeting of the Academy he shall present such matters as the Council may determine.

Article 3. He shall attend the meetings of the Academy and the Council and shall arrange for the keeping of a faithful record of the attendance and of the proceedings. In the absence of the President and of all the Vice-Presidents, he shall call the meeting to order and preside until a chairman is chosen by majority vote of the Fellows present.

Article 4. He shall apprise officers and committees of their election or appointment, and inform the Treasurer and the Chairman of each Standing Committee of appropriations of money voted by the Academy.

Article 5. He shall notify all persons who may be elected Fellows or Foreign Honorary Members, send to each a copy of the Statutes, and on their acceptance issue the proper Diploma. After all elections, he shall insert in the Records the names of the Fellows by whom the successful nominees were proposed.

Article 6. He shall keep and cause to be printed annually a list of the Fellows and Foreign Honorary Members, arranged in their several Classes and Sections, and a list of Fellows and Foreign Honorary Members of whose deaths he has been informed.

Article 7. He shall arrange for the preservation of records of the death of Fellows and Foreign Honorary Members and biographical notices published on the occasion of their death, or at other times.

See Chap. i, art. 2; chap. ii, art. 4; chap. iii; chap. iv, art. 1, 3, 4, 5; chap. ix, art. 3; chap. xi, art. 1 (iii), 2; chap. xii, art. 1, 3.

CHAPTER VII

THE TREASURER AND THE TREASURY

ARTICLE 1. The Treasurer shall collect all money due or payable to the Academy and all gifts or bequests made to it. He shall pay all bills due and payable by the Academy when approved by the proper officers. He shall sign all leases of real estate in the name of the Academy. He shall be the official custodian of all bonds, stocks and other securities and, with the written approval of any one member of the Committee on Finance, he shall have full authority to sell and transfer, invest and reinvest from time to time in such manner and upon such terms as shall to him seem best, the whole or any part of the personal property of the said Academy.

He shall keep a faithful account of all receipts and expenditures, submit his accounts annually to the Auditing Committee, and render them at the expiration of his term of office, or whenever required to do so by the Academy or the Council.

He shall keep separate accounts of the income of the Rumford Fund, and of all other special Funds, and of the appropriation thereof, and render them annually.

He shall fund all payments received in commutation of Dues, their income only to be applied toward current expenditures.

His accounts shall always be open to the inspection of the Council.

ARTICLE 2. He shall report annually to the Council at its March meeting on the expected income of the various Funds and from all other sources, together with appropriations needed by Officers and Standing Committees for the ensuing fiscal year. He shall also report the names of all Fellows who may be then delinquent in the payment of their Annual Dues.

ARTICLE 3. He shall give such security for the trust reposed in him as the Academy may require.

ARTICLE 4. With the approval of a majority of the Committee on Finance, he may appoint an Assistant Treasurer to perform his duties, for whose acts, as such assistant, he shall be responsible; or, with like approval and responsibility, he may employ any Trust Company doing business in Boston as his agent for the same purpose, the compensation of such Assistant Treasurer or agent to be fixed by the Committee on Finance and paid from the Funds of the Academy.

ARTICLE 5. At the Annual Meeting he shall report in print all his official doings for the preceding year, stating the amount and condition of all the property of the Academy entrusted to him, and the character of the investments.

ARTICLE 6. The Financial Year of the Academy shall begin with the first day of April.

ARTICLE 7. No person or committee shall incur any debt or liability in the name of the Academy, unless in accordance with a previous vote and appropriation therefor by the Academy or the Council, or sell or otherwise dispose of any property of the Academy, except cash or invested funds, without previous consent and approval of the Council.

See Chap. ii, art. 2, 6, 7, 8; chap. iv, art. 1; chap. vi, art. 4; chap. ix, art. 6; chap. xi, art. 1 (i), (iv), (v), art. 2; chap. xii, art. 1.

CHAPTER VIII

THE LIBRARIAN AND THE LIBRARY

ARTICLE 1. The Librarian shall have charge of the Library and keep a correct catalog of it.

ARTICLE 2. The Librarian shall have authority to expend such sums as may be appropriated by the Academy for the purchase, repair, or maintenance of books, periodicals, etc., and for defraying other necessary expenses connected with the Library.

ARTICLE 3. The Librarian shall have the custody of the publications of the Academy. With the advice and consent of the President, he may effect exchanges with other associations.

See Chap. iv, art. 1; chap. xi.

CHAPTER IX

THE EDITOR AND THE PUBLICATIONS

ARTICLE 1. The Editor shall have charge of the conduct through the press of the publications of the Academy. Together with the Committee on Publication he shall determine the contents of the publications.

ARTICLE 2. The publications of the Academy shall be as follows:

(i) The Proceedings shall be published at least semi-annually as soon as may be possible after the Annual May Meeting, and the stated December meeting next following, and shall contain a record of each stated or special meeting of the Academy. They shall be known respectively as the Summer and Winter numbers of the Proceedings.

The Summer number of the Proceedings shall include reports of the officers and standing committees for the preceding year; a list of the officers, councillors and members of standing committees elected at the preceding Annual Meeting; and such other matter as the Publication Committee may approve.

The Winter number of the Proceedings shall include a current list of officers, councillors, standing committees, Fellows and Foreign Honorary Members; the Statutes of the Academy; the Act of Incorporation of 1780 and its amendments; and such biographical notices or other matter as the Committee on Publication may approve.

In the discretion of the Committee on Publication, interim numbers of the Proceedings may be issued for the publication of accepted serial papers or other scholarly material.

(ii) Memoirs, monographs and volumes of collected papers may be published from time to time.

(iii) The Bulletin of the American Academy of Arts and Sciences shall be published eight times each year preceding the stated meetings, containing notices of such meetings, communications from the Council or Officers, and such other matter as may be of timely interest to the Fellows.

ARTICLE 3. A copy of the Summer and Winter numbers of the Proceedings shall be mailed to each Fellow, Fellow Emeritus, and Foreign Honorary Member.

A copy of the Bulletin shall be mailed to each Fellow and Fellow Emeritus, and Foreign Honorary Member.

A copy of any Interim number of the Proceedings shall be mailed only to those Fellows, Fellows Emeriti, and Foreign Honorary Members, who shall make written request to the Secretary for that number.

ARTICLE 4. Fellows who pay Annual Dues or who are exempted from further payment thereof under Chapter II, Article 7, Fellows Emeriti, and Foreign Honorary Members shall be entitled, upon written request to the Librarian, to receive gratis one copy of each number of Proceedings, Memoirs, and Bulletin which have been issued after their election and are available.

ARTICLE 5. Not more than two hundred extra copies of each paper printed in the Proceedings shall be placed at the disposal of the author without charge.

ARTICLE 6. The Editor shall have the authority to expend for printing and other expenses of publication such sums as may be appropriated by the Academy for such purposes; also such sums as may be made available to him by the Council from any source for particular publications under the sponsorship of the Academy.

See Chap. iv, art. 1; chap. xi, art. 1 (vi).

CHAPTER X

THE COUNCIL

ARTICLE 1. The Council shall exercise general supervision over all affairs of the Academy not explicitly reserved to the Academy as a whole.

It shall consider all nominations of Fellows and Foreign Honorary Members duly sent to it by the Committee on Membership, and act upon them in accordance with the provisions of Chapter III.

With the consent of the person concerned it shall have power to transfer in respect to status, Class, or Section.

ARTICLE 2. Nine members shall constitute a quorum.

ARTICLE 3. It shall act upon all resignations and forfeitures of membership in the Academy.

It shall appoint all agents and subordinates not otherwise provided for by the Statutes, prescribe their duties, and fix their compensation. They shall hold their respective positions during the pleasure of the Council.

It shall fill any vacancy caused by death, resignation or incapacity of any officer.

ARTICLE 4. It may authorize the appointment for terms not exceeding one year, and prescribe the functions of such committees of its number or of the Fellows of the Academy, as it may deem expedient, to facilitate the administration of the affairs of the Academy or to promote its interests.

ARTICLE 5. At the stated March meeting of the Academy it shall recommend for action at that meeting the appropriations which in its opinion should be made for the ensuing fiscal year, and the Annual Dues therefor.

It may recommend special appropriations at any Stated Meeting of the Academy, or at a Special Meeting, in the call for which such business shall have been included.

See Chap. ii, art. 2, 10; chap. iii, art. 1, 2; chap. iv, art. 1, 2; chap. v, art. 2, 3; chap. vi, art. 2, 3; chap. vii, art. 1, 2, 7; chap. ix, art. 6; chap. xi, art. 1; chap. xii, art. 1, 4, 6.

CHAPTER XI

STANDING COMMITTEES

ARTICLE 1. At each Annual Meeting, the following Standing Committees shall be perpetuated by election by ballot by the Fellows of the appropriate number of nominees to maintain the strength of each committee as provided hereinbelow. A Fellow shall not be eligible for election for two successive terms.

(i) *The House Committee* shall consist of three Fellows, one of whom shall be elected at each Annual Meeting to serve for a term of three years. One member of this committee shall in such an election be designated by the electors to serve as Chairman of the House Committee for the duration of the term for which he has been elected as a member of the committee. It shall have general charge of maintaining the House of the Academy in suitable condition for the uses thereof approved by the Council.

The Chairman of the House Committee or his designate shall approve in writing all expenditures within its authorized budget or special appropriations for repairs, services, supplies, or operation of the House, including compensation of House employees.

The House Committee, in consultation with the Treasurer, shall determine the equitable proportion of expense to be assessed for the use of the facilities of the House which have been approved by the Council for other than Academy activities.

(ii) *The Committee on Membership* shall consist of the President, *ex officio*, as Chairman, and eight other Fellows, one of whom from each of the four Classes shall be elected annually to serve for a term of two years. It shall have the duties designated to it in Chapter III.

(iii) *The Committee on Meetings* shall consist of the President as Chairman, the Secretary, who shall act as secretary of the committee, and the four Vice-Presidents, *ex officiis*, together with four other Fellows, one from each Class, two of whom shall be elected annually to serve for a term of two years. It shall arrange for meetings of the Academy.

(iv) *The Committee on Finance* shall consist of the Treasurer as Chairman, *ex officio*, and four other Fellows, two of whom shall be elected annually to serve for a term of two years. It shall have general supervision of the investments of the Academy.

(v) *The Auditing Committee* shall consist of two Fellows, one of whom shall be elected annually to serve for a term of two years. It shall audit the accounts of the Treasurer with power to employ an expert and to approve the payment of his charges.

(vi) *The Committee on Publication* shall consist of the Editor as Chairman, *ex officio*, and four other Fellows, one from each Class, two of whom shall be elected annually to serve for a term of two years. It shall have the authority and the responsibility of determining the contents and of effecting the printing of the publications of the Academy as set forth in Chapter IX.

(vii) *The Permanent Science Fund Committee* shall consist of seven Fellows of whom there shall be elected at Annual Meetings in each triennium, respectively, two, two, and three members, each to serve for a term of three years. One member of this Committee shall, in such an election, be designated by the electors to serve as Chairman for the duration of the term for which he has been elected as a member of the Committee. It shall review all applications for grants addressed to it and shall from time to time recommend to the Council appropriate disbursements from the income received by the Academy from the Trustee of the Permanent Science Fund, for carrying out the purposes set forth in the Agreement and Declaration of Trust which governs the use of this income.

(viii) *The Rumford Committee* shall consist of seven Fellows of whom there shall be elected at Annual Meetings in each triennium, respectively, two, two, and three members, each to serve for a term of three years. One member of this Committee shall, in such an election, be designated by the electors to serve as Chairman for the duration of the term for which he has been elected as a member of the Committee. It shall invite applications for pecuniary assistance in support of researches in the general fields of heat and light broadly interpreted, and shall from time to time recommend to the Council disbursements in support thereof from the income from the Rumford Fund. Biennially it shall recommend to the Council a candidate for the reception of the Rumford Medal to be awarded in accordance with the Rumford trust, and shall in general see to the proper execution of this trust.

(ix) *The C. M. Warren Committee* shall consist of seven Fellows of whom there shall be elected at Annual Meetings in each triennium, respectively, two, two, and three members, each to serve for a period of three years. One member of this Committee shall, in such an election, be designated by the electors to serve as Chairman for the duration of the term for which he has been elected as a member of the Committee. It shall invite applications for pecuniary assistance from any person wishing to engage in research in any branch of chemistry, and shall recommend to the Council such applications as seem worthy of aid and such other disbursements from the income of the C. M. Warren Fund as it deems appropriate to the advancement of research in chemistry.

(x) *The Amory Prize Committee* shall consist of seven Fellows, each of whom shall serve from the time of his election until his successor is elected. Immediately after the Annual Meeting in 1950 and at least every seventh year thereafter it shall at the call of its senior officer, meet and elect from its own members, a chairman and a secretary, each to serve for seven years or until his successor is elected in a similar manner at an interim meeting at which at least five members shall cast their votes. For each septennium beginning with that which was concluded on November 10, 1933, it shall recommend to the Council for its approval a recipient or recipients of the Amory Prize and a gold medal or other token of honor and merit to be conferred upon each by the President and Fellows of the Academy in recognition of an invention or other contribution in the medical field specified in and according to the terms of the bequest of Francis Amory.

(xi) *The Nominating Committee* shall consist of twelve Fellows, three from each Class, having the right to vote. The members of the first Nominating Committee constituted hereunder shall be appointed within each Class by the President for terms of one, two and three years. The President shall also appoint one of the Committee to act as Chairman. Thereafter, as vacancies in the membership of the Committee occur, four members, one from each Class, shall be elected annually to serve for a term of three years, one member at such election being designated to act as Chairman for the duration of the term for which he has been elected as a member of the Committee. Nominations for such elections, within each Class, shall be by a vote of the Committee.

ARTICLE 2. Each Standing Committee shall confine its recommendations and its expenditures to such sum in each fiscal year as shall have been notified to its Chairman by the Secretary of the Academy as appropriations voted by the Academy, or by the Treasurer as income available for its purposes.

ARTICLE 3. Each Standing Committee shall report to the Academy at the Annual Meeting its acts of the previous year.

See Chap. iii; chap. iv, art. 1, 3; chap. vi, art. 4; chap. vii, art. 1, 2, 4; chap. ix, art. 1, 2.

CHAPTER XII

Meetings, Communications, and Amendments

Article 1. There shall be annually eight Stated Meetings of the Academy, namely, on the second Wednesday of October, November, December, January, February, March, April, and May. Only at these meetings, or at adjournments thereof regularly notified, or at Special Meetings called for the purpose, shall appropriations of money be made or amendments of the Statutes be effected.

The Stated Meeting in May shall be the Annual Meeting of the Corporation.

Special Meetings shall be called by the Secretary at the request of the President, of the Council, or of ten Fellows having the right to vote; and notifications thereof shall state the purpose for which the meeting is called.

The Council shall have authority, as occasion may demand, to arrange additional meetings and to cancel any of the statutory meetings, except that meetings for transacting business shall be held as required by the Statutes.

Article 2. Except as otherwise provided, twenty-five Fellows having the right to vote shall constitute a quorum for the transaction of business at Stated or Special Meetings. Eighteen Fellows shall be sufficient to constitute a meeting for literary or scientific communications and discussions.

Article 3. Upon the request of the presiding officer or the Secretary, any motion or resolution offered at any meeting shall be submitted in writing.

Article 4. Fellows may introduce guests at any of the literary or scientific meetings of the Academy.

Article 5. All amendments to the Statutes, whether proposed by Fellows or by the Council, shall be considered by the Council and reported with recommendations for action to the Academy. At a subsequent Stated Meeting, or at a Special Meeting called for the purpose, the notice for which in either case shall state this proposed amendment, the Academy shall act upon the amendment. Two-thirds of the Fellows present, in a meeting of not less than forty Fellows, must vote in the affirmative to enact the amendment.

See Chap. ii, art. 6, 10; chap. iii, art. 1, 2; chap. iv, art. 1, 3, 4, 5; chap. v, art. 1; chap. vi, art. 2, 3; chap. vii, art. 2, 5; chap. ix, art. 2; chap. x, art. 5; chap. xi, art. 1, 3.

CHARTER OF INCORPORATION

An Act to incorporate and establish a Society for the cultivation and promotion of Arts and Sciences. Granted May 4, 1780, by an Act of the Legislature of Massachusetts, and amended by the Acts of 1910, 1911, 1931, and 1947.

As the Arts and Sciences are the foundation and support of agriculture, manufactures, and commerce; as they are necessary to the wealth, peace, independence, and happiness of a people; as they essentially promote the honor and dignity of the government which patronizes them; and as they are most effectually cultivated and diffused through a State by the forming and incorporating of men of genius and learning into public societies for these beneficial purposes.

Be it therefore enacted by the Council and House of Representatives in General Court assembled and by the authority of the same, that [sixty-two persons][1] be, and they hereby are formed into, constituted, and made a body politic and corporate, by the name of THE AMERICAN ACADEMY OF ARTS AND SCIENCES, and that they, and their successors, and such other persons as shall be elected in the manner hereafter mentioned, shall be and continue a body politic and corporate, by the same name forever.

And be it further enacted by the authority aforesaid, that the Fellows of the said Academy may from time to time elect a President, one or more Vice-Presidents, one or more Secretaries, and such other officers of the said Academy as they shall judge necessary or convenient; and they shall have full power and authority from time to time to determine and establish the names, number, and duties of their several officers, and the tenure or estate they shall respectively have in their offices; and also to authorize and empower their President, or some other Fellow of the Academy, at their pleasure, to administer such oaths to such officers as they shall appoint and determine, for the well-ordering and good government of the said Academy, provided the same be not repugnant to the laws of this State.

And be it further enacted by the authority aforesaid, that the Fellows of the said Academy shall have one Common Seal, which they may make use of in whatsoever cause or business shall concern the Academy, or be relative to the end and design of its institution; and shall have power and authority from time to time to break, change, and renew the Common Seal, at their pleasure; and that they may sue and be sued, in all actions, real, personal, and mixed, and prosecute and defend the same unto final judgment and execution, by the name of the President and Fellows of the American Academy of Arts and Sciences.

---

[1] For the names of the Fellows incorporated, see Memoirs, Vol. XI, Part I, pp. 33, 34.

And be it further enacted by the authority aforesaid, that the Fellows of the said Academy may from time to time elect such persons to be Fellows thereof, as they shall judge proper, and that they shall have full power and authority from time to time to suspend, expel, or disfranchise any Fellow of the said Academy who shall by his conduct render himself unworthy of a place in that body, in the judgment of the Academy; and also to settle and establish the rules, forms, and conditions of election, suspension, expulsion, and disfranchisement.

And be it further enacted by the authority aforesaid, that the Fellows of the said Academy shall have full power and authority from time to time to make and enact such reasonable rules, orders, and bylaws, not repugnant to the laws of this State, as shall be necessary or convenient for the well-ordering and good government of the said Academy, and to annex reasonable pecuniary fines and penalties to the breach of them, not exceeding the sum of *twenty pounds,* to be sued for and recovered in any court of record within this State, in the name and for the use of the President and Fellows of the said Academy; and the same rules, orders, and bylaws to repeal at their pleasure; and also to settle and establish the times, places, and manner of convening the Fellows of the said Academy; and also to determine the number of Fellows which shall be present to constitute a meeting of the said Academy. *Provided,* that the Fellows of the said Academy shall meet twice in a year at the least; and that the place of their meeting shall never be more than thirty miles distant from the town of Boston.

And be it further enacted by the authority aforesaid, that the Fellows of the said Academy may, and shall forever hereafter, be deemed capable in the law, of having, holding, and taking in fee-simple, or any less estate, by gift, grant, devise or otherwise, any lands, tenements or other estate real and personal. *Provided,* that the said real estate shall not exceed in value the sum of *two hundred thousand dollars,* and the said personal estate shall not exceed in value the sum of *five hundred thousand dollars,* all the sums mentioned in the preceding section of this act to be valued in silver at the rate of *six shillings and eightpence* by the ounce. And the annual interest and income of the said real and personal estate, together with the fines and penalties aforesaid, shall be appropriated for premiums to encourage improvements and discoveries in agriculture, arts, and manufactures, or for other purposes consistent with the end and design of the institution of the said Academy as the Fellows thereof shall determine.

And be it further enacted by the authority aforesaid, that the end and design of the institution of the said Academy is, to promote and encourage the knowledge of the antiquities of America, and of the natural history of the country, and to determine the uses to which the various natural productions of the country may be applied; to promote and encourage medical discoveries; mathematical disquisitions; philosophical inquiries and experiments; astronomical, meteorological, and geographical observations; and improvements in agriculture, arts, manufactures, and commerce; and, in fine, to cultivate every art and science which may tend to advance the interest, honor, dignity, and happiness of a free, independent, and virtuous people.

And it is further enacted, that the place where the first meeting of the Fellows of the said Academy shall be held shall be the Philosophy Chamber in the University of Cambridge; and that the Honorable James Bowdoin, Esq., be, and he hereby is authorized and empowered to fix the time for holding the said meeting, and to notify the same to the Fellows of the Academy.

81—4

Proceedings of the American Academy of Arts and Sciences

Vol. 81, No. 4, P. 165–251 — March, 1952

THE RESISTANCE OF 72 ELEMENTS, ALLOYS AND COMPOUNDS TO 100,000 KG/CM²

By P. W. Bridgman

CONTENTS

Introduction 169

Apparatus and Method 169

Detailed Presentation of Data 184

- Lithium 184
- Sodium 185
- Potassium 188
- Rubidium 189
- Caesium 190
- Copper 192
- Silver 192
- Gold 194
- Beryllium 194
- Calcium 195
- Strontium 197
- Barium 199
- Magnesium 200
- Zinc 200
- Cadmium 201
- Mercury 202
- Aluminum 205
- Gallium 205
- Indium 207
- Thallium 207
- Lanthanum 208
- Cerium 213
- Praseodymium 214
- Neodymium 215
- Titanium 216
- Zirconium 216
- Thorium 217
- Carbon (Graphite) 218
- Silicon 219
- Germanium 221
- Tin 221
- Lead 222
- Vanadium 222
- Columbium 224
- Tantalum 224
- Arsenic 224
- Antimony 226
- Bismuth 228

Chromium 230
Molybdenum 231
Tungsten 232
Uranium 232
Selenium 232
Tellurium 233
Manganese 235
Iron 236
Nickel 237
Cobalt 238
Rhodium 239
Palladium 239
Iridium 240
Platinum 240
Intermetallic Compounds 241
Iron-Nickel Alloys 242
Silver Sulfide 244
Copper Sulfide 244
Molybdenite 245

Summary and Conclusion 245

THE RESISTANCE OF 72 ELEMENTS, ALLOYS AND COMPOUNDS TO 100,000 KG/CM^2

By P. W. Bridgman

Received November, 1951

Introduction

In extending the range of measurement of electrical resistance at room temperature from 30,000 to 100,000 kg/cm^2 a radical change in technique is demanded. At room temperature there are no longer any liquids available with which to transmit pressure, for there is no substance ordinarily liquid which does not freeze at a pressure of 30,000 or less, and the permanent gases like nitrogen or argon freeze below 50,000 The only possibility is helium, which by extrapolation may be expected to freeze at 100,000, but the technical difficulties seemed so prohibitive in the view of experience with it to only 7,000 that its use was not even considered. This means that pressure must be transmitted with a solid, which means that the pressure will not be truly hydrostatic. It then becomes a problem to find a solid with as low a plastic yield point as possible and to find such a geometrical design for the apparatus that the effects of the unavoidable departures from a hydrostatic stress distribution are minimized.

Apparatus and Method

It was very soon evident that a solution could not be found by modifying the former apparatus with which extensive volume measurements have been made to 100,000.[1] This apparatus was completely supported on the outside surface by a liquid carrying hydrostatic pressure; that is, an inner pressure vessel nesting inside an outer vessel was used. It seemed hopeless to get electrically insulated leads into the inner vessel in the small space available, to say nothing of other difficulties.

Many preliminary attempts were made. The guiding idea of a number of these was to generate the pressure in a chamber of the proportions of a thin disc, so that the frictional effects at the edges would be a comparatively small part of the total stress load over the faces. The piston was made in the shape of a short boss on a massive block, in this way exploiting the support afforded by the surrounding unloaded parts. Carboloy was used in all this construction because of its superior strength. Much of the preliminary work was done with pressure chambers in the form of shallow depressions in the face of massive blocks. Such attempts to use a pressure chamber evacuated from the block were abandoned because of the fractures which invariably started from the corners of the depression, where there are stress concentrations which are particularly harmful with a substance as brittle as carboloy.

During the preliminary work the possibility of using glass as a pressure transmitting solid was extensively investigated and for the first time measureably large plastic flow with permanent increase of density was observed in glass at room temperature. A disc of commercial lime glass 0.25 inch in diameter and 0.006 inch thick was compressed between two flat carboloy blocks to a mean stress of 100,000 kg/cm^2. Under this pressure there was a permanent decrease of thickness of 22 per cent, and a permanent increase of density from 2.497 to 2.617, that is, by 4.8 per cent. The glass, although copiously cracked, was not reduced to powder but was broken into clear blocks of the order of a millimeter or somewhat less on a side. Professor E. P. Larsen was so kind as to make an optical examination and found marked double refraction, with an increase of mean index for Na light from 1.515 to 1.527. Prolonging the time of loading does not perceptibly increase the permanent alteration, so that the behavior is *not* that of a viscous liquid. That is, under these conditions glass work hardens.

Another interesting effect was observed with glass. The carboloy blocks between which the glass was compressed were insulated from each other and connected to the same Kipf and Zonen high sensitivity short period galvanometer used in the final measurements. When pressure was increased to the fracture point of the glass, as indicated by the noise, the galvanometer was thrown into continuous spasmodic deflections to both sides of zero. The fracture of the glass would seem to be accompanied by electrostatic effects, perhaps due to intermolecular charges, and these give statistically a net charge fluctuating between positive and negative by a Brownian motion which is recorded by the galvanometer. It is not inconceivable that more detailed study of this effect would yield quantitative information on the intermolecular charges.

The method finally adopted is indicated in Figure 1. Two carboloy blocks, A and B, 1 inch in diameter and supported on the outside by shrunk-on steel rings C and D, are ground to have a flat central disc 0.5 inch in diameter with conical shoulders making an angle of 4° with the flat face. The 0.5 inch faces of the carboloy blocks are separated by a cell 0.010 inch thick, carrying the specimen whose resistance is to be measured embedded in the solid with which pressure is transmitted. Pressure is produced by pushing the blocks together in a hydraulic press; a very sturdily constructed press with piston diameter of 8 inches was used. Approximately 400 kg/cm^2 on the 8 inch piston suffices to produce 100,000 kg/cm^2 on the carboloy faces. The normal crushing strength of carboloy under simple compressive stress is in the neighborhood of 60,000 kg/cm^2. The greater stress attained here is a consequence of the load being confined to only a portion of the block, so that the surrounding parts afford support for the highly stressed central disc. This principle of construction has been used in some of my other work.[2]

The solid medium transmitting pressure within the cell is AgCl. Measurements of the plastic flow strength under pressure[3] had already indicated that it is among the best solids available for this purpose. Preliminary experiments had also indicated that it is a sufficiently good insulator under the conditions for the use contemplated, that is, for the resistance of metals. The effect of pressure is to increase the resistance of AgCl. Dummy runs showed that no appreciable error was introduced under any of the conditions of this paper by conductivity of the AgCl. There would be serious difficulty, however, if an attempt were made to find the resistance of most semi-conductors by unmodified use of the same technique. AgCl is convenient for other reasons than its low plastic yield point and high electrical resistance. Its transparency is advantageous in properly position-

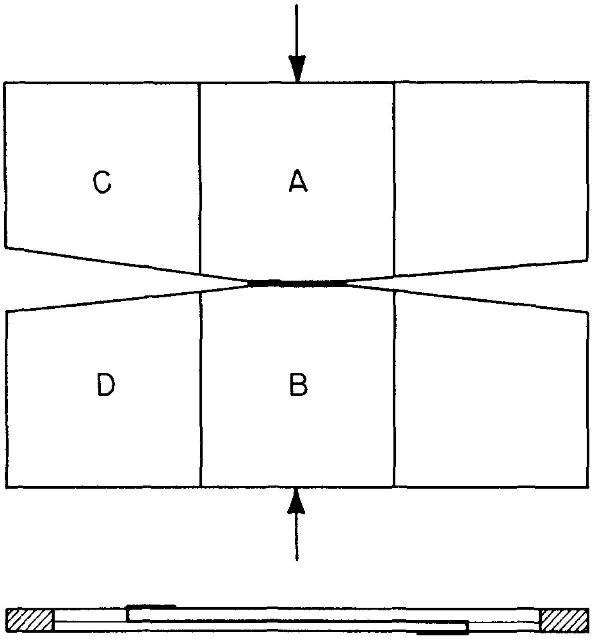


FIGURE 1. The apparatus for applying pressure to the resistance specimen. An enlarged section of the cell is shown in the lower part of the diagram.

ing the resistance specimen within the cell, and it can be readily manipulated mechanically by squeezing out to thin discs of the required thickness, from which the discs are cut with a punch. The material used was single crystal stock which had been prepared by the Harshaw Chemical Company. I am indebted to Professor J. C. Street for making available for use in these experiments a rather large stock of this material which he had originally obtained for use in cosmic ray detectors.

The AgCl is prevented from leaking from the cell by an outer ring of pipestone (catlinite), a material which I have extensively used in insulating the leads in former measurements to 30,000. The pipestone seals the cell by friction. The outer diameter of the ring is 0.500 and the inner diameter 0.407 inch, making the width of the pipestone 0.047 inch. This, with a thickness of 0.010, ensures the maintenance of the seal by friction at any pressure provided only that the coefficient of friction is greater than 10/94 or 0.11. The effective coefficient of friction is materially higher than this and very little trouble was experienced from blowing out of the AgCl through the pipestone. Blow outs were further inhibited by subjecting the carboloy blocks to a preliminary application of pressure with less than the normal amount of AgCl in the cell. The result of the preliminary application is that a shallow annular impression is permanently left in the face of the carboloy; on future applications this helps to retain the pipestone from blow-out. The annular depression gradually deepens with continual use, until eventually the carboloy has to be refigured if it has not previously cracked. Contrary to my previous experience in measuring compressibilities to 100,000, I found that under these conditions carboloy work hardens to an appreciable extent, and the groove in carboloy blocks which have been refigured several times deepens much more slowly than in the virgin block.

The blocks may fracture in two characteristic ways: by radial cracks running down the conical shoulders but not in the flat faces, and by spalling off of flat slabs, sometimes in the flat face but more often from the periphery immediately at the boundary of the flat part. In many cases the radial cracks are not deep but are merely the cracks at the edges of thin flakes which spall off. The fracture of these blocks would make an interesting study for their own sake. In general, the fractures were as if the region immediately under the flat faces were being intruded bodily by the pressure into the massive block, in which it produces expansion and tensile cracks. This spalling off may well be due to expansion on release of load. Contrary to expectation, the harder grades of carboloy proved best for this purpose. *Grade 905 was best with 883 next. The softer 779 and 55A were* not so good.

Considerable work was done to find the best way of getting electrical leads into the cell. At first the attempt was made to take the leads out through the pipestone sides, using thin flat strips of metal sandwiched between two pipestone rings. An advantage of such a procedure would have been that several leads could have been used, making possible resistance measurements by a potentiometer method, or the simultaneous measurement of several resistances, one of which could have been employed as a pressure gauge analogous to the manganin pressure gauge of former work. No successful method of doing this was found, the leads always being blown out by the internal pressure. The reason is doubtless the much lower plastic flow point of a crystalline metal as compared with the amorphous pipestone. Eventually the simplest possible method was adopted, the leads being taken out through the face of the AgCl discs, one making mechanical contact with one carboloy block and the other with the other. The resistance measured was the resistance between the two carboloy blocks, which was almost entirely constituted by the resistance of the specimen. For this measurement one of the carboloy blocks had to be insulated from the rest of the press; this was satisfactorily accomplished with mica 0.005 thick on the thrust surface of the block and a thin cylindrical shell of hard rubber on the cylindrical surface.

The resistance of the specimens in the cell was of the general order of 0.01 ohm. This was measured in the simple way suggested in Figure 2.

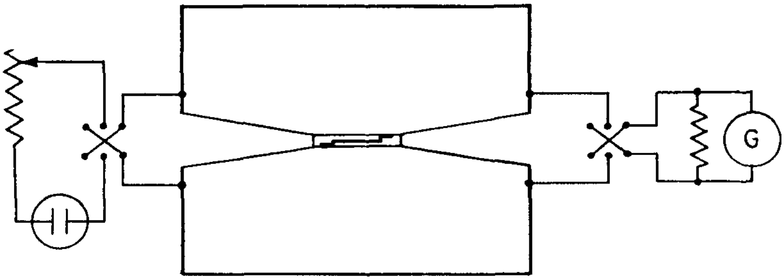


FIGURE 2. Scheme of the method of measuring resistance.

The blocks were tapped for current and galvanometer terminals. A suitable current from a dry cell was passed through the specimen from one block to the other and the difference of potential between the ends of the specimen (that is, between the blocks) determined from the deflection of a galvanometer in the potential circuit. For this a Kipf and Zonen short period galvanometer was used, of 56 ohms internal resistance, with 70 ohm shunt. Correction of the galvanometer deflections for failure of linearity with voltage was determined by special calibration. The controlling resistance in the current circuit in general varied from 150 (the lowest in any of the set-ups) to several thousand ohms. For any one set-up this resistance was held constant. Constancy of the dry cell during the run was checked by direct determination of the galvanometer deflection with dry cell in series with a megohm before and after each run. Reversing switches in both galvanometer and current circuits permitted elimination of the effect of parasitics by taking the average of the four galvanometer deflections for the four switch combinations. Galvanometer readings were consistent and reproducible to 0.1 mm on a total deflection of approximately 10 cm, that is, to one part in a thousand. It will be seen that with the usual range of resistances the galvanometer deflections are proportional to the resistance between the blocks, that is, to the resistance of the specimen except for block and contact resistances. If the resistance of the specimen becomes too high, proportionality fails. The situation may then be met by more elaborate calculation, or if the resistance gets up into the order of ohms, as it sometimes did for special substances like tellurium and selenium, the circuits have to be rearranged. Any such rearrangement by combinations of series and shunt resistances is sufficiently obvious not to need detailed description.

With resistances as low as 0.01 ohm the most potent source of error proved to be contact resistance between the specimens and the blocks, and much time was spent in finding how to deal with it. It was fairly satisfactory to use intermediate electrodes between the specimen and the blocks of annealed gold 0.001 inch thick, but a completely satisfactory solution was finally found by using intermediate electrodes of annealed copper 0.001 inch thick, freshly amalgamated just before use and wiped to remove superfluous mercury. Blank runs with such electrodes gave values for the terminal resistances closely equal to the values roughly calculated by the conventional formulas (given by Jeans for example) for the resistance between a disc shaped electrode in the surface of a semi-infinite conductor and the body of the conductor. Geometrical irregularities made impossible an exact comparison of the measured contact resistance with the theoretical value, but in any event contact resistance was got down to a small fraction of the resistance of the balance of the specimen, and the method of correction was such that any influence of such resistance should correct out, as will appear in the future discussion.

The general method of mounting the resistance specimen in the cell and making contact with the electrodes is indicated in Figure 3. The cell is composed of two AgCl discs. The specimen, usually in the form of a single straight bar, is sandwiched between the two discs. Contact at the two ends is by means of amalgamated copper strips, threaded through radial slots cut in the disc in such a way as to be in contact with the specimen on the inside and the carboloy block on the outside. The dimensions of the specimen varied with the material from 0.010 to 0.040 or even more inches wide, and from 0.001 to 0.004 inches thick. The length between the copper terminals was of the order of 0.18 inch. In order to avoid deformation of the specimen as far as possible when pressure is applied, the discs were preformed, after inserting the copper electrodes by stamping into them in a die a depression to take the specimen. Because of the difficulty of making depressions in the two opposing discs of AgCl accurately enough to take the specimen without offsetting, the depression for the specimen was made in only one disc, which was made appropriately thicker to bring the center of the specimen to the center of the sandwich.

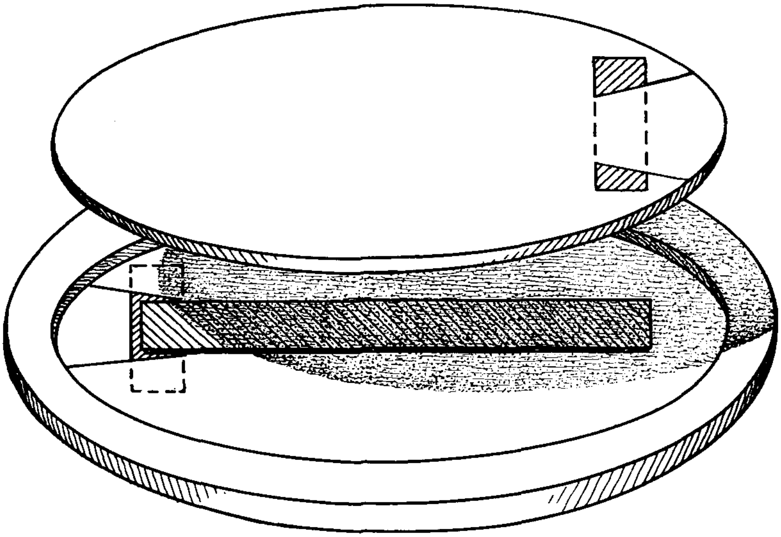


FIGURE 3. Details of the method of mounting the resistance in the cell.

In order to avoid blow outs and at the same time permit as uniform a distribution of pressure as possible over both pipestone ring and AgCl the quantity of AgCl must be carefully controlled. In practise the pipestone ring was made first, by cutting off in the lathe a slice from a tube of pipestone machined to the correct inside and outside diameter. The thickness of a pipestone ring cut off in this way cannot easily be controlled to closer than 0.0003 inch. After cutting off its thickness is measured with an Ames gauge, and then the thickness of the AgCl disc adjusted to somewhat better than 1 per cent by weighing and scraping to the value indicated by previous experience. The initial thickness of the AgCl prepared in this way was about 0.001 inch less than the pipestone, ensuring that pressure makes up first on the pipestone and seals the AgCl within. After assembling the resistance specimen in the AgCl sandwich, it is subjected to a final forming by pressing between flat pistons in a die, and then mounted in the pipestone ring, which it enters friction tight. It is then mounted between the carboloy blocks in a special subpress which is mounted as a whole in the 8 inch press. During mounting the disc is held concentric with a paper collar.

There is chemical action between many metals and AgCl, which may be decomposed with deposition of free silver. Such chemical action was completely suppressed in all cases by varnishing the surfaces of contact with a thin toluene solution of a resin which I owe to the courtesy of the Dupont Company. Not only did use of this resin eliminate chemical action but it also made marked, in some cases dramatic, improvement in freeing the stresses transmitted by the AgCl to the specimen of harmful shearing components. Without the use of the varnish certain metals were almost invariably torn completely apart after a pressure run, whereas with the varnish the only permanent effect was a small permanent increase of resistance with correspondingly small permanent change of dimensions. In consequence, the resin was used with all specimens, whether or not there was possible chemical decomposition.

The process of applying and releasing pressure is irreversible. The crushing strength of pipestone is comparatively small and its compressibility is nearly the same as that of AgCl, which, under 100,000 kg/cm^2, loses about 16 per cent in volume. These requirements rather severely limit the materials possible for the ring. When pressure is applied the pipestone crushes down, following the volume change of the AgCl within the cell, so that pressure is more or less uniformly distributed over the entire flat face. But on release of pressure the crushed pipestone cannot follow back beyond its elastic range, which however appears surprisingly large under these conditions, so that eventually the pipestone ring begins expanding radially, pushed by the expanding AgCl within. This terminates sometimes in a blow out, although usually not, but in all cases terminates in an irreversible permanent radial stretching of the AgCl disc and the embedded specimen. The result is that acceptable resistance measurements can be made only on the increasing branch of the cycle, and this means incidentally that readings can be made only on the initial application of pressure. A preliminary pressure seasoning, such as was previously regularly employed up to 30,000, is therefore impossible. However, this should be of little consequence with pressures as high as these, which are presumably much beyond any initial internal stresses in the material. Readings were as a matter of fact always made with decreasing pressure as well as with increasing, but such readings could be used only by way of check or confirmation of questionable points.

An important preliminary question was to what extent the pipestone ring supports the load and prevents transmission of the average pressure to the AgCl within. Information about this afforded by the resistance measurements themselves was ambiguous because of uncertainty in the correction to be applied for non-hydrostatic components in the stress, as will appear presently in the discussion. Fortunately an independent method of answering this is afforded by the polymorphic transitions which are exhibited by a number of the materials measured here. At such a transition there may occur discontinuities in resistance, and by locating the discontinuity the ostensible pressure of the transition may be determined. To be suitable for this purpose it is necessary that the transition run sharply with little or no over-shooting, and that the accompanying discontinuity of resistance be large. Fortunately there are at least two such transitions, the I–II transition of bismuth at approximately 25,000 kg/cm^2, and the I–II transition of thallium at approximately 45,000. Checks on the pressure within a few per cent were obtained at both these points. The agreement was within the uncertainty due to the other corrections about to be discussed, and in all calculations the pressure was taken at its ostensible value, directly calculated from the areas and the pressure on the low pressure end of the 8 inch press. This latter was controlled with a dead weight gauge.

The procedure consisted in successively adding increments of weight to the load on the dead weight gauge and making the four galvanometer readings. The dimensions are such that no time was required for dissipation of thermal effects after each change of load, and a run could be rapidly made, always in less than an hour. For the first study of a new material, readings were made with 23 equi-spaced pressure points with increasing pressure and about half as many with decreasing. If the first run disclosed no incidents requiring closer examination subsequent runs might use only 11 points with increasing pressure and 5 with decreasing. In this case the points were more closely spaced at the lower end. There were perceptible initial effects due to the crushing of the pipestone; for this reason the first reading was always made at 5,500 kg/cm^2. Under this pressure the pipestone was sufficiently crushed and the electrodes were pressed into sufficiently close contact with the carboloy so that there were no further low pressure irregularities.

As an indication of the smoothness and regularity of the readings there is shown in Figure 4 the directly determined readings for tungsten, resistance in terms of galvanometer deflection in centimeters, against pressure in terms of the weight on the pan of the dead weight gauge. Tungsten is one of the metals for which greater irregularities than average might be anticipated, because of its high elastic limit, permitting the accumulation of high non-hydrostatic pressure components of resistance change. In the following detailed presentation of data the range of resistance change for the four runs is given, permitting further estimation of the probable accuracy.

The most serious problem connected with the use of the new technique is to devise a method for correcting for the non-hydrostatic components of stress. We may set as the final object of the measurements to determine the effect of hydrostatic pressure on the resistance of a specimen with electrodes rigidly attached to its surface, the electrodes experiencing the same deformation under pressure as the body of the specimen. It was this that was measured previously up to 30,000 kg/cm^2 on specimens exposed to truly hydrostatic pressure. This may be called the "direct" resistance, and the corresponding pressure coefficient the "direct pressure coefficient." If it is desired to find the effect of pressure on specific resistance, the direct coefficient may be converted to coefficient of specific resistance with the help of the cubic compressibility, which is known by previous measurement for most of the substances of this paper. This

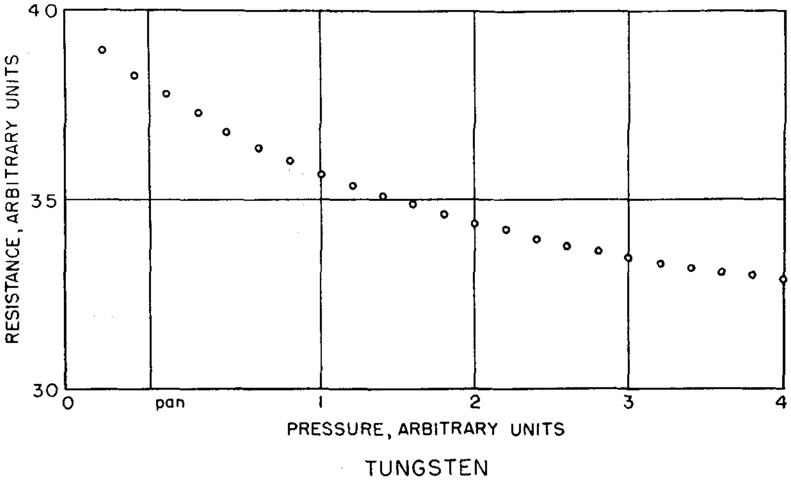


FIGURE 4. Uncorrected readings of the resistance of tungsten. Ordinates are resistance in arbitrary units (galvanometer deflection) and abscissae pressure in arbitrary units (weights on the dead weight piston gauge).

conversion can be made uniquivocally only for substances electrically isotropic, that is, for substances crystallizing in the cubic system.

The conditions in the present experiments differ from previous conditions in the failure of the stress to be hydrostatic. This failure results in two sorts of effect; specific effects of shearing components of stress on resistance and gross overall effects due to change in the geometrical shape of the specimen under the distorting effect of applied shearing stress. These two effects may each be considered within the elastic and the plastic range.

Comparatively little is known about the effect of shearing stress on resistance. There are measurements on the effect of simple tension in the elastic range on a number of metals[4] and it is known that there is no rule about even the sign of the effect, for examples are known of both increase and decrease of resistance under tension. There seems to be no connection between the sign of the tension effect and the sign of the normal hydrostatic pressure effect. The presumption is therefore that under the conditions of these experiments components of shearing stress may either increase or decrease resistance. Less is known about the behavior of resistance during plastic flow. There are a number of experiments on the permanent change of resistance produced after permanent stretching and release of stress, but apparently nothing has been done to find the effects during plastic deformation while the deforming stress is still acting, which are the conditions here. The natural presumption is that in the plastic range changes in specific resistance are of minor importance, but preliminary experiments which I have undertaken indicate that the situation is not simple. Another factor to be considered is to what extent the elastic range of distortion is extended by mean hydrostatic pressure in these experiments. Experiments on steel have indicated an increase in the elastic limit under tension of between 15 to 30 per cent under a pressure of 30,000 kg/cm^2. It is not unreasonable to expect under present conditions extensions of the elastic range of 100 per cent or more under top pressure. The result of all these considerations is that it is impossible to make any general statement about the intrinsic effect of shearing stresses in these experiments, but that effects of either sign may be anticipated. The magnitude of such effects is probably a not large fraction of the true hydrostatic effects because 100,000 kg/cm^2 is so far in excess of the normal shearing flow stress for the materials measured here.

The gross over-all distortion of the specimens due to being embedded in the AgCl is complicated and compounded from effects arising from distortion of the carboloy parts and volume compression of the AgCl. Consider first the ideal case in which the carboloy is infinitely rigid so that the plane faces of the cell remain plane under pressure and with unaltered diameter, and the only distortion is a thinning of the cell because of the volume compression of the AgCl. To a first approximation, assuming a low plastic yield stress for the AgCl, the distortion of the AgCl consists of a shrinkage perpendicular to the face of the disc of an amount three times the normal linear shrinkage under the mean hydrostatic pressure, but no change in the direction of the two dimensions of the face. This distortion is that produced by a mean hydrostatic stress on which is superposed a mean two dimensional distending stress in the plane of the faces expanding the surface dimensions back to initial. This two dimensional distending stress is afforded by friction in the faces and may be assumed comparatively small in view of the low plastic flow stress. Embedded in the AgCl deformed in this way is the specimen. To a first approximation the stress acting on it is hydrostatic, and the first effect is to produce in the specimen its normal hydrostatic compression, under which the specimen shrinks uniformly in all directions. But the specimen is more or less effectively embedded in the AgCl and is therefore dragged out in the dimensions of the face and correspondingly thinned. The net effect is that the specimen is longer and of smaller cross section than it would be if subjected to pure hydrostatic pressure. Both of these distortions increase the resistance. Hence if the specimen is one of the normal metals for which the effect of pressure is to decrease resistance the change of resistance under pressure as determined in this experiment will be too small in so far as it is affected by this effect. Conversely, if the pure hydrostatic effect is to increase resistance, too large an increase of resistance will be found in these experiments. The corrections for this effect may readily be worked out in terms of the assumed known compressibility. It will be found as a matter of fact that the effect is in accordance with expectations with regard to sign for a number of the softer metals, which are also the more compressible metals, provided those metals have a normal negative pressure coefficient of resistance. But this correction gives the correct sign for the abnormal metals with positive pressure coefficient only in one doubtful instance, and then with very poor numerical agreement. The conclusion would seem to be that the effect due to dragging of the specimen by differential compressibility is of minor importance.

A much more potent source of deformation than differential compressibility arises from the deformation of the carboloy blocks. The solution, credited to Hertz, is well known for the deformation of a semi-infinite block loaded over a circular disc on its face by a uniformly distributed pressure. Under such a distributed pressure the circular disc is depressed at the center, or in other words, the cell between the two carboloy blocks assumes the shape of a double convex lens. Calculation shows that the curvature of this lens becomes very appreciable at the top pressure. The lenticular distortion of the carboloy may be demonstrated experimentally by replacing the cell of pipestone and AgCl with a solid disc of lead. The plastic strength of lead is so low that it may be assumed that it exerts a uniformly distributed pressure on the carboloy block. On applying pressure to such a lead disc, lead is extruded until the edge thins down to practically nothing and the lead trapped in the interior takes the form of a double convex lens of thickness at the center of 0.0028 inch, or nearly 30 per cent of the initial thickness of the cell. This thickening at the center is accompanied by a flow of AgCl toward the center, which carries the specimen with it to a certain degree, thickening and shortening it. This deformation decreases the resistance, which is thus a change in the opposite direction from that arising from the differential compressibility. It is a fact that most of the metals, particularly those with small pressure coefficients, show in these experiments larger decreases of resistance than would be expected from former measurements. The lens effect may be presumed to be the major effect in most cases. It cannot be calculated, since the conditions from one experiment to another vary too much. Slow progressive deformation of the carboloy due to sinking of the pipestone groove may be mentioned as one such factor.

Another cause of imperfect reproducibility is the crystalline structure of the AgCl. The original material was usually one crystal and even after it had been pressed into discs of the proper thickness it retained a large degree of unicrystallinity, as shown by the slip lines often evident on the surface after an experiment. Internal slip in the AgCl would tend to deform the specimen, and in a way depending on the particular orientation; this is not a reproducible factor.

The indirect evidence is that the deforming effect of the AgCl is much less than it would be expected to be if the specimen were rigidly embedded in the AgCl and dragged with it without slip. Rather, most metals slip through the AgCl with comparatively little drag, and the stresses are much more nearly hydrostatic than I feared at first or indeed was indicated by the preliminary experiments. The use of the resin varnish has a very large ameliorating effect in suppressing drag. There is one rather specific piece of evidence with regard to the small amount of drag. It is known that AgCl has a polymorphic transition at 90,000 kg/cm^2, with 1.6 per cent decrease of volume.[5] If the drag between the specimen and AgCl is large it would be expected that all specimens would show some incident in the pressure-resistance relation in the neighborhood of 90,000. Actually the smoothness of the resistance curve was disturbed in only a very few cases, and these were for the softest metals, such as sodium, where it would be expected. It would appear that most metals become stiffer than AgCl under pressure and slip through it with little drag.

In spite of serious attempts, no satisfactory way was found of combining these various effects into a single theoretically determined correction for converting the changes of resistance as measured to the effect of a pure hydrostatic pressure. The only course was to put the correction on an empirical basis, determined in such a way as appeared most intrinsically reasonable in the light of all the data, and also determined so as to correct the values to the known values previously found in the range up to 30,000 kg/cm^2 for pure hydrostatic pressure. Fortunately this latter information is available for nearly all the substances of this paper.[6] The "intrinsically reasonable" may appear to be a poor guide in penetrating into new territory, where a large part of the element of adventure consists in finding effects that were not anticipated, that is, that were not "intrinsically reasonable." However, there can be no question that physics does have to proceed in some such way in getting into new territory, and in the following discussion of the most plausible way of determining the corrections considerations will appear *in parvo* typical of what has been done by others on a grander scale many times in the past.

In the first place, in view of the many problematic factors introduced by stress propagation by a solid, more measurements were made on each substance than usual. Four sets of measurements have been made on most of the materials of the following. This involved altogether more than 300 set-ups and runs. The dimensions of the four specimens were usually varied so as to produce variation in the presumptive corrections; usually two different widths were used. For each substance two different sets of carboloy blocks were used, of different grades of carboloy and with geometrical variations which might rise to as much as 15 per cent on the thickness arising from variations in the pipestone rung. No consistent differences were found that could be associated with the grade of carboloy (this involves the elastic constants of the carboloy) or with geometrical differences. In fact, no consistent factor of variation of any sort was found. Certain substances were much more suggestive in giving presumptive information about the best correction than others. For instance, if the absolute value of the pressure effect is small, and if the correction is capricious, it would be expected that one run might give a positive sign for the pressure effect and another a negative sign. Such behavior was almost never found; the substances with small effects showed consistently the same sign on all set-ups. The same sort of thing was shown by substances with flat maxima or minima. These always occurred at approximately the same pressure with different set-ups. Or if the curve of resistance against pressure reverses curvature, it was found that the pressure of reversal was the same for different set-ups, and therefore presumably of real significance. It was therefore assumed that all such effects were real and were not to be removed by the correction, as could easily happen. For instance, a correction linear in pressure is capable of removing a flat minimum.

The problem was therefore, how to find a correction which would produce agreement with the former results up to 30,000 and not remove the special features disclosed at higher pressures, of which there were an unexpectedly large number. In general it was not safe to extrapolate the present readings to zero pressure, the lowest readings being at 5,000 with usually obvious irregularities at the lowest pressures. The best range for comparing the present with former values was between 20,000 and 30,000, not infrequently running the present values down to 10,000. In many cases, particularly for those substances with absolutely small effects, it seemed safe to run the former results by extrapolation to 40,000 or even in a few cases to 50,000 for comparison with present results. The simplest process of comparison was to take the ratio of present to former resistance increments, from 30,000 as the fiducial pressure, and to plot these ratios as a function of pressure. If the ratios were approximately constant, well and good, and the present measurements in the new range were corrected by this constant. If the ratios were not constant, as was usually the case, they might show comparatively little variation and might vary in such a way that a curve could be plausibly extrapolated.

In general, selection of the most plausible correction was made from a few simple types of correction. Of these the simplest are additive and multiplicative corrections. It is to be distinguished whether the addition or multiplication is to be performed on the total resistance or on the change of resistance from some fiducial point. Each type of correction has its specific effect in altering such characteristic features of the original measurements as maxima or minima or points of inflection, and choice of the type of correction was made with this in mind. The type of correction also depends on the estimate made of the probably dominant factor in the correction. For instance, if the dominating factor in the correction is the effect of shearing stresses and if it may be assumed that these remain in the elastic range, then the correction will be a multiplicative correction on the measured change of resistance which will be proportional to the pressure. On the other hand, if the dominant effect is due to change of geometrical shape due to drag and plastic deformation, then the effect is independent of the specific effect of pressure on resistance for the particular substance and is an additive correction on the total resistance, this additive correction being a definite fraction of the total resistance at any given pressure and itself being a function of pressure to be determined if possible from the low pressure comparison. If the shearing stress effect is one which is elastic in the low pressure range but becomes plastic at the upper end of the range, then the presumption is that there is little change in the correction at the upper end and the correction curve may be extrapolated graphically to an asymptote at the upper end. Such an asymptotic extrapolation was employed in a number of cases. It would not be feasible to reproduce all the arguments by which the method of correction was determined in every individual case; any notable features will be mentioned in the following under the detailed description of the different materials. The whole process was a tentative one of feeling one's way, but feeling one's way in a manner suggested by the whole body of experience with all the experimental material (the final results were not calculated until all the experimental material for all substances was in), which would be difficult to communicate or justify to another, and which will have to be accepted as my opinion as to the most probable values. Fortunately the corrections are not large enough to obscure the essential qualitative nature of the results, and in a number of cases closely concordant results were obtained by applying different methods of correction.

The detailed presentation of data now follows. Elements are given first, arranged in chemically similar families (columns of the periodic table) beginning with low atomic weights, that is, with the alkali metals. Elements are followed by "intermetallic compounds," and these by an unsystematic collection of a number of iron-nickel alloys, and finally three semi-conducting compounds.

Detailed Presentation of Data

Lithium. Because of their chemical activity in air all the alkali metals require special methods of handling during mounting in the cell. Lithium is the least active of these and was easiest to handle; the same general method was used for all. The specimen was squeezed to the right thickness and cut to final dimensions under a heavy oil. It was then washed under petroleum ether or pentane. The dummy cell had meanwhile been prepared with pipestone and AgCl discs of proper dimensions. The cell was then mounted in a glass cylinder with rubber stoppers at the two ends through which passed two pistons and a vacuum connection. The cell, in place on the flat face of one piston, was filled with pentane and the specimen placed in the cell without losing protection by pentane during the process of transfer. The upper stopper of the glass cylinder was inserted, and the pentane evaporated with a vacuum pump. After evaporation the cell was closed by pushing in the upper piston through the stopper and consolidated in an arbor pressure before removing from the cylinder. The seal between the two AgCl discs thus produced was always adequate, even for so reactive a substance as caesium. The resin varnish played an essential role in securing this seal.

The lithium was from my former stock, with which all my previous measurements have been made. The purity has been proved to be adequate. Eight different set-ups and runs were made. The first four runs were made before all the details of the technique of preforming the AgCl to the shape of the specimen had been worked out. This preforming proved to be specially necessary for the softer metals like lithium, as shown by certain irregularities in the first runs not found in the later ones. The first four runs were discarded in the final result. All four final runs agreed in showing certain unusual qualitative features with regard to curvature. The resistance of lithium is known to increase initially under pressure with upward curvature. All four present runs showed this initial upward curvature, but less markedly than the former measurements. This curvature reversed in the neighborhood of 20,000 to concave downward, followed again by a point of inflection and reversal to upward curvature in the neighborhood of 60,000, from which up to 100,000 the upward curvature becomes continually more pronounced.

The directly measured proportional changes of resistance between 30,000 and 100,000, taken from 30,000 as fiducial, ranged from 0.232 to 0.301, mean 0.261. This proportional change of resistance was about 33 per cent less below 30,000 than given by former measurements.[7] It suggests itself that the difference is chiefly due to flow of AgCl toward the center because of lenticular thickening of the cell; such an effect would be expected to be large for the softer metals. The best way of correcting to secure agreement with former values is more difficult to decide here than often because the reverse curvature now found shows that it is not safe to extrapolate the former results beyond the range of measurement. The course adopted, which has the advantage of preserving the characteristic reversals of curvature, was to apply an additive correction proportional to pressure. This is nearly equivalent to applying a multiplicative correction of 1.5 to ΔR, but this latter would have obscured the reversals of curvature. The result of applying the correction was to change an uncorrected proportional resistance of 1.169 to 1.239 at 30,000, and from 1.475 to 1.704 at 100,000. The final results are shown in Table I and in Figure 5.

Three of the four final runs showed a disturbance above 90,000 doubtless associated with the transition of AgCl. The numerical value of the disturbance varied considerably, but in sign it was a decrease of resistance.

Sodium. The material was stock used in former measurements. Mechanical softness gave no particular difficulty in handling during setting up the

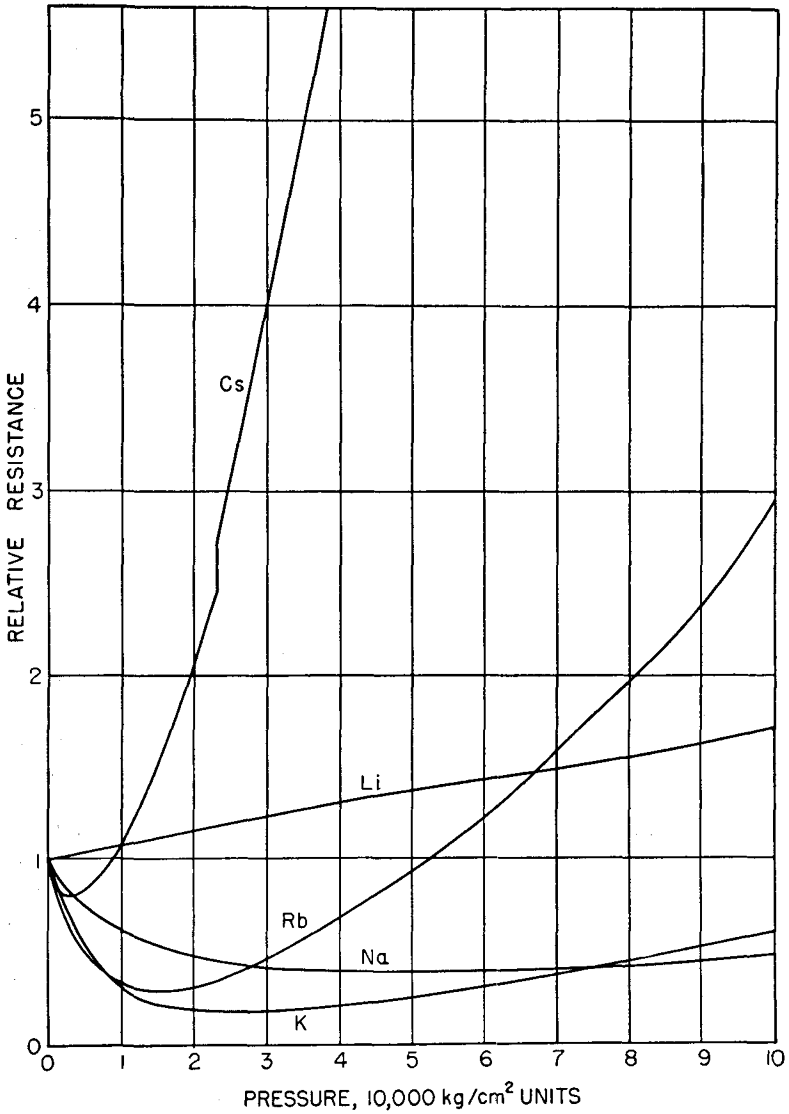


FIGURE 5. The relative resistances of the five alkali metals as a function of pressure.

TABLE I

Pressure kg/cm^2	R/R$_0$				
	Lithium	Sodium	Potassium	Rubidium	Caesium
0	1.000	1.000	1.000	1.000	1.00
10,000	1.073	.618	.303	.326	1.09[a]
20,000	1.152	.483	.182	.309	2.12
30,000	1.239	.413	.178	.455	4.20[b]
40,000	1.305	.392	.203	.695	5.96
50,000	1.366	.390	.252	.924	8.90
60,000	1.428	.399	.309	1.218	9.32[c]
70,000	1.487	.412	.370	1.594	7.55
80,000	1.551	.430	.439	1.972	6.55
90,000	1.623	.452	.514	2.38	5.88
100,000	1.704	.479	.596	2.95	5.33

[a] Minimum of 0.80 at 2.500
[b] Transition at 22,000; resistances 2.44 and 2.70
[c] Maximum of 11.2 at 54,950 kg/cm^2

cell. Four runs were made, all in close agreement. There is a minimum in the resistance in the general neighborhood of 45,000, here observed for the first time. Such a minimum was anticipated because of the known minima at less than 30,000 of potassium, rubidium, and caesium.

The measured changes of resistance below 30,000 are of the general order of 15 per cent less than given by former measurements to 30,000.[8] This is in the direction to be expected from the effect of differential compressibility. Because the compressibility of sodium is high, it would be expected that these runs would be capable of giving particular information about the validity of the correction for differential compressibility. Accordingly a rather elaborate formulation and application of this correction was made. The correction is proportional to $v^{2/3}$. The result of applying the correction yielded such improbable results that it was rejected. The correction resulted in displacing the minimum to 80,000 and in flattening it almost to the vanishing point. The internal evidence is that the minimum is to be taken nearly at its face value, because the minima found for potassium and rubidium are at the same pressures as found previously. Accordingly a mathematically simple scheme of correction was adopted which preserves the features felt to be qualitatively correct. This was approximately equivalent to multiplying the proportional changes of resistance, taken from 20,000 as fiducial, by a constant factor. The uncorrected and the final results are shown in Figure 6. The correction is such that the corrected and the uncorrected curves will cross at a little above 100,000, where the resistance has again the value at the arbitrary selected fiducial point, here at slightly less than 20,000 kg/cm^2. This fiducial point can have no absolute significance, but except for this the method of correction was the most plausible that I could devise.

The final resistances in terms of the resistance at atmospheric pressure as unity are given in Table I. The minimum is in the neighborhood of 46,000. Previous measurements to 30,000 had suggested by extrapolation a minimum in the neighborhood of 40,000.

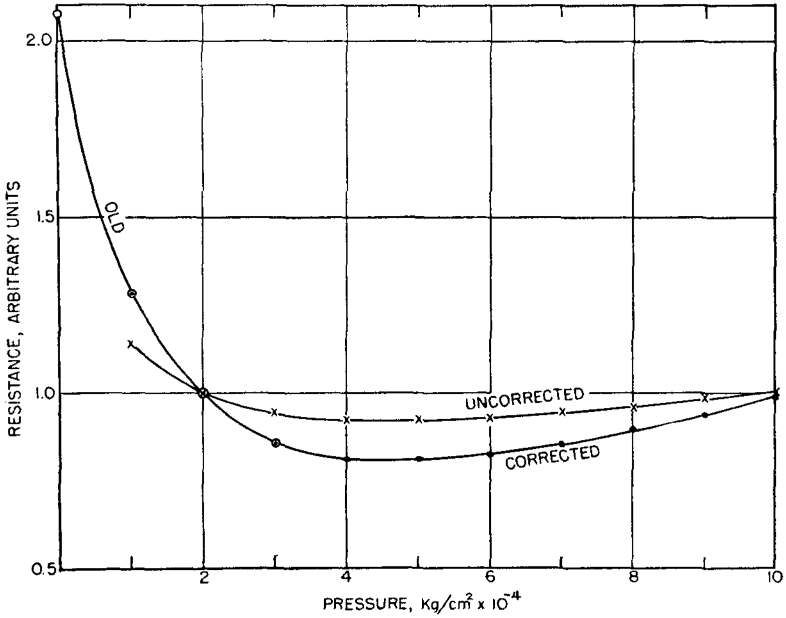


FIGURE 6. The resistance of sodium, showing in one diagram the former values to 30,000 kg/cm^2, and the present uncorrected and corrected values to 100,000 kg/cm^2.

Potassium. The material was from former stock, kept sealed under nujol. Manipulation was beginning to get troublesome because of mechanical softness and chemical activity. Four runs were made with qualitatively good agreement. All runs showed a sharp minimum in resistance at a pressure not differing by more than 1,000 kg/cm^2 in either direction from 25,000 kg/cm^2, against a previously observed value of 25,400. On the other hand, the magnitude of the relative changes of resistance below 30,000 were much less than formerly.[9] Divergence in this direction would be expected because of differential compressibility, which is markedly larger for potassium than for sodium. The best method of correcting the present values to agree with the former is particularly problematical here because of the minimum in the middle of that range, 20,000 to 30,000, which for most materials affords the most secure basis for the comparison and the reduction. Correction was finally made by multiplying the observed changes of proportional resistance taken from 20,000 as fiducial by the constant factor 2.56, this being the factor required to correct the present values to agree with the former ones at 10,000. The final values so corrected are shown in Table I and Figure 5. It must be recognized that there is unusually large uncertainty in the upper end of the range. There can be no question I think, however, of the qualitative features of unusually marked upward curvature and the fact that at 100,000 resistance is still considerably short of rising back to the initial value at atmospheric pressure.

Potassium showed more markedly than lithium or sodium a disturbance in resistance between 90,000 and 100,000 presumably due to the transition of AgCl. In all cases the resistance dropped somewhat. The values given in the table were obtained by smooth extrapolation from 90,000 to 100,000.

Rubidium. This was old stock, kept sealed under nujol. It had been previously purified by slow distillation. This metal was so soft mechanically that a modification of the technique applicable to the first three alkalies proved necessary. The dimensions of the specimen had to be accurately controlled to fit the depression preformed in the AgCl. Too little material may result in pinching through the specimen when applying initial pressure to the cell, because with this excessively soft material the situation is entirely dominated by flow in the AgCl. Too much material results in its extrusion between the two AgCl discs, sometimes with short circuiting between the carboloy blocks. The method adopted was to extrude the rubidium to wire of a diameter selected so that when cut to length and squeezed flat it just filled the preformed pocket. The diameter of the wire meeting these requirements was 0.013 inch, squeezed flat to about 0.004 inch thick.

Four runs were made with good qualitative agreement. All runs showed a minimum resistance near 20,000 kg/cm^2, against 17,000 found before. The best method of securing agreement with previous results [10] was made even more difficult to determine than for potassium by the presence of the minimum in the range of most effective comparison. There was the further complication that the former results were not for the free specimen, but were for the specimen as encased in a glass capillary. These former results for the constrained specimen were first corrected to the free specimen by assuming an average value for the compressibility of the glass envelope. The correction changes the former value for the proportional resistance at 30,000 from 0.576 to 0.454. The correction finally adopted involved a correction of about 40 per cent on the total resistance and a correction on the *change* of resistance by a factor of 2. It would probably be difficult to justify this in detail, but the method does have the virtue of giving agreement with the former values to 30,000, and preserving the two not very pronounced points of inflection shown consistently by all four runs. These points of inflection are similar qualitatively to the points of inflection shown by lithium, and although not so pronounced are presumably real. The final values are given in Table I and Figure 5. It will be noticed that the final resistance at 100,000 has risen to be markedly larger than the initial resistance at atmospheric pressure, unlike potassium and sodium. The difference between the three metals in this regard changes progressively as is to be expected.

Caesium. The material was former stock, stored sealed in glass in *vacuum*, not in oil. It had been purified by slow distillation to have a sharp freezing point and to show a silvery color in contrast to the usual golden color due to dissolved oxide. The mechanical difficulties in manipulating this substance proved to be really formidable, and I was glad to have three usable runs after many attempts. Not only is the mechanical softness extreme, but the melting point is so close to room temperature that it was necessary to work with precooled tools and in a cooled room. Worst of all, the chemical activity is so great that it attacks liquids effective with the other alkalies in preventing action by air during assembly. Action by pentane was rapid and even with pure i-octane the action was too rapid. It was apparently not known to the organic chemists that caesium attacks these purified hydrocarbons. It was finally found that the action with nujol was slow enough to offer prospects of success, but this demanded a change in technique of assembly which was not easy. The caesium, extruded into wire, was transferred to position in the cell while still wet with nujol, and formed in its preformed seat with a spatula by hand. The nujol had to be then removed with blotting paper, precisely controlling the removal so as to leave a film of nujol thick enough to prevent chemical action by air during the few minutes of the remaining manipulations, and not so thick as to prevent the two AgCl discs from sticking together on consolidating the cell in the arbor press. Once sealed, the resin varnish was completely effective in preventing further chemical action, but on release of pressure after the run, the caesium broke out of the cell by differential expansion, acting essentially like a liquid, and was speedily destroyed by chemical action with the air.

The most important question to be answered by the measurements was the nature of the resistance change on going through the recently discovered electronic transition at 45,000, a transition in which the crystal lattice is unaltered, but in which there is a large change of volume.[11] Three usable runs were made; two of these gave acceptable values only above 30,000, there being some unexplained disturbance at low pressures presumably connected with geometrical changes in the configuration forced by the AgCl. At higher pressures the internal evidence is that the caesium becomes stiff enough to dominate, and readings of all three runs showed a gratifying smoothness above 30,000. Only one of the three runs showed a slight disturbance above 90,000, probably associated with the polymorphic change in AgCl. Previous experience with the effect of pressure on the melting points would prepare one to anticipate such a high pressure stiffening, since at high pressures the order of melting points in the alkali series reverses, caesium having the highest melting point and lithium the lowest.

The one run which gave good values below 30,000 gave smooth values down to 5,000, exhibited the low pressure minimum in this neighborhood, and at the previously known lattice transition[12] from body centered to face centered cubic near 23,000 exhibited a discontinuity of resistance with the unusual increase of resistance already known. All three runs agreed qualitatively above 30,000 in showing a rapid rise to a sharp cusp within 1,000 kg/cm^2 of 55,000, followed by a smooth drop with marked upward curvature to 100,000. Because of its importance, readings were made with decreasing pressure in the same small steps as with increasing pressure and the same behavior found, namely a sharp cusp, rising to practically the same height as with increasing pressure. The pressure of the cusp with decreasing pressure was lower, however, by nearly 20,000 kg/cm^2. The mean pressure of the cusp with increasing and decreasing pressure varied in the three runs from 43,000 to 44,000 kg/cm^2, nearly the same as the pressure of the electronic transition, 45,000, previously found. One might be inclined to infer from this that the directly observed pressures with increasing and decreasing pressure differ by friction by the amount of this difference. This I believe would be a quite unjustified conclusion in view of the other evidence, for instance, the agreement of the known transition pressures for bismuth and thallium, the position of the minima for potassium and rubidium, and the fact that for caesium itself the lattice transition occurs at approximately the correct pressure. It seems to me that most of the difference at the electronic transition between increasing and decreasing pressure is to be ascribed to real effects — most transitions run only with appreciable transgression of the thermodynamic equilibrium values and the amount of transgression is greater the smaller the specimen. The situation is complicated here by the unknown effect of the non-hydrostatic stresses in displacing the thermodynamic transition.

The three runs were in rather good numerical agreement; the relative resistances at the cusp in terms of the resistance at 30,000 were 2.24, 2.23, and 2.11. The three runs were simply averaged. The best method of reduction to secure agreement with former values is not so simple. In the first place the former values were not for the free metal, but for caesium in glass. The former values were first corrected for the glass, as had also been done for rubidium. At 20,000 this amounted to a correction on the proportional resistance of approximately 10 per cent. It was then considered how to correct to displace the cusp from 55,000, its present value, to 45,000 as given by former volume measurements as the location of the electronic transition. This seemed a hazardous undertaking, particularly in view of the probability that most of the 10,000 displacement is real, and I did not attempt it. The simple course was adopted of multiplying all changes of resistance from 20,000 as fiducial by that constant factor which would secure agreement between present and previous results in the common range 20,000 to 30,000, and letting the cusp fall as it would. The factor of correction was 1.35, smaller than for the other alkali metals.

The results so corrected are shown in Table I and in Figures 5 and 7. Any theoretical use made of these results will have to contemplate a possible shift of the pressure of the cusp of as much as 10,000 kg/cm^2 toward lower values, with such a consequent readjustment of the course of the entire curve as seems most plausible to the theoretician. The most surprising result of the present measurements is that the electronic transition is manifested as a *cusp* in the resistance, whereas the volume measurements gave it as a discontinuity in the volume (transition of the first kind). It is a task for the theoretician to discover whether so large a qualitative difference, extending over such a wide range of pressure, can be plausibly explained as an effect of internal strains in a substance as soft as this.

Copper. This was electrolytic copper of 99.9999 per cent purity, rolled to 0.001 inch thick and annealed at red heat in vacuum after forming. Four runs were made, with 10 per cent extreme variation of the resistance at 100,000 in terms of the resistance at 30,000 as fiducial. Below 30,000 the presently found changes of resistance are larger than the former values,[13] the correction factor running from 0.8 at the extrapolated zero to 0.9 at 40,000, extrapolated from the former results to 30,000. The correction above 40,000 was determined by a smooth free hand graphical extrapolation of the lower values to 100,000 in such a way as to approach unity asymptotically at 100,000. The precise method of extrapolating the correction factor is here of little importance; the resistance at 100,000, taking the observed values at their face value with no correction, is 0.856 at 100,000 against 0.866 given by the correction procedure outlined above The results are shown in Table II.

Silver. The material was old Kahlbaum "K" sheet stock. It was rolled to 0.001 inch thick and annealed to red heat in vacuum after forming. Four runs were made, with two different grades of carboloy, and two widths of specimen (0.010 and 0.020 inch). The extreme values for the propor-

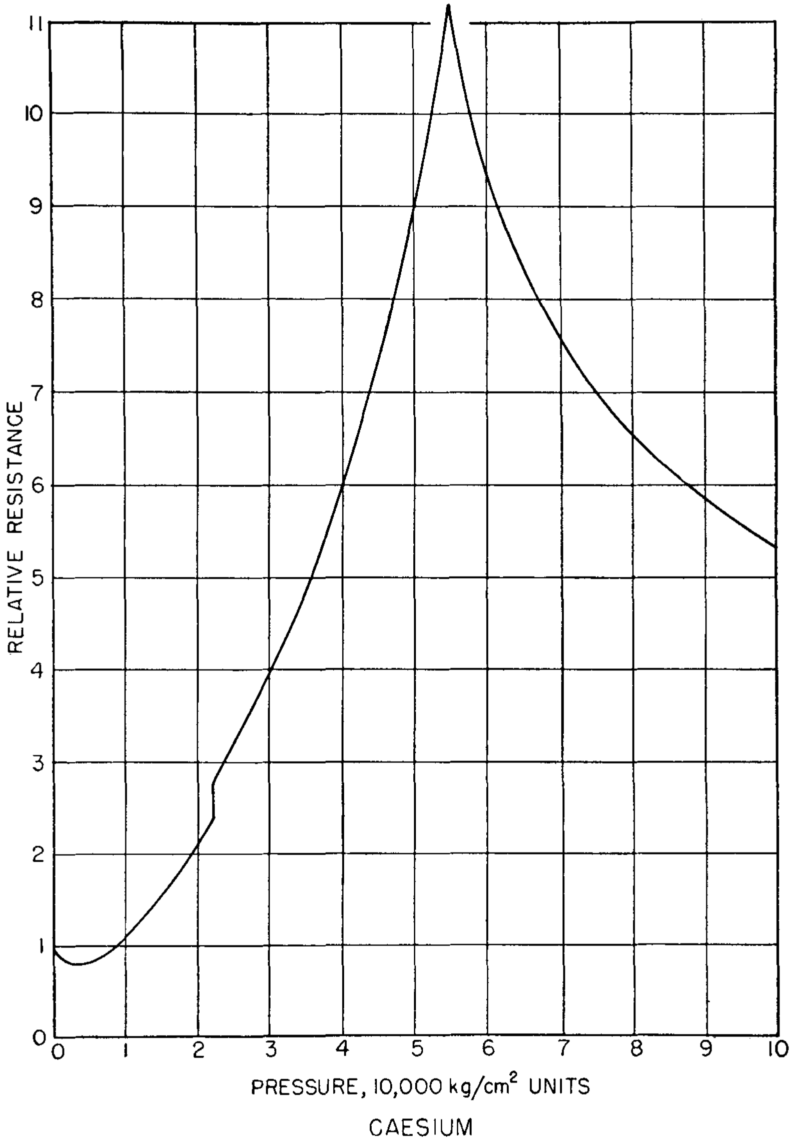


FIGURE 7. The relative resistance of caesium to 100,000 kg/cm^2.

tional change of resistance at 100,000, in terms of the resistance at 30,000 as fiducial, were 0.112 and 0.123. The four results were simply averaged. Compared with previous results[14] the changes of resistance were a few per cent less than those previously found. The difference as compared with copper is in the direction to be accounted for by the greater differential compressibility of silver. The correction between 30,000 and 100,000 was obtained by drawing a smooth free hand correction curve running to unity at 100,000. The maximum value of the correction on the change of resistance was 8 per cent. The results are shown in Table II.

TABLE II

Pressure kg/cm²	R/R_0		
	Copper	Silver	Gold
0	1.000	1.000	1.000
10,000	.981	.966	.971
20,000	.965	.936	.944
30,000	.949	.910	.919
40,000	.934	.885	.896
50,000	.920	.864	.874
60,000	.907	.846	.855
70,000	.895	.831	.840
80,000	.884	.819	.829
90,000	.875	.809	.821
100,000	.866	.802	.816

Gold. The material was from Baker and Company, specified as their "highest purity" but with no analysis. It was rolled from 0.010 wire to 0.001 inch thickness and annealed. Four runs were made, with two grades of carboloy and in two widths, 0.010 and 0.020 inch. The proportional changes of resistance at 100,000 in terms of the resistance at 30,000 as fiducial were 0.117, 0.118, 0.116 and 0.101 for the four runs. These four results were simply averaged. Compared with previous results,[15] the present proportional changes of resistance are too large below 30,000 and somewhat too small at 40,000 (former result extrapolated). A smooth correction curve was drawn up to 100,000, with the limiting correction of unity at 100,000. The maximum correction on the change of resistance was 8 per cent. The precise correction procedure is of little moment; the resistance at 100,000 with no correction was 0.803 against 0.816 given as final in Table II.

Beryllium. The material I owe to the courtesy of the Brush Beryllium Company of Cleveland, who supplied me with a hot rolled sheet 0.004 inch thick. I had previously made unsuccessful attempts to machine a piece of suitable dimensions from the cast rod on which my former measurements[16] had been made; the difficulty with this material is its mechanical hardness and its fragility in small thicknesses. The hot rolled sheet as furnished had a highly oxidized and mechanically irregular surface. It was filed or ground to thickness varying from 0.003 to 0.0015 inch for different specimens and widths varying from 0.020 to 0.030 inch. Three successful runs were made with two grades of carboloy. A fourth attempt failed because of a blow-out at 36,000 with increasing pressure, practically the only blow-out that has occurred under such conditions. The reason for this was that the carboloy block had been allowed to go too long without refiguring. Up to the point of blow-out the results on the fourth run agreed with the other three.

The observed decrement of proportional resistance at 100,000 in terms of the resistance at 20,000 as fiducial as found on the three runs was 0.085, 0.0917 and 0.0954; these results were simply averaged.

Although the resistance of beryllium had been previously measured[17] to 30,000, because of the presumptive difference between the former cast material and the present hot rolled sheet, the measurements to 30,000 were repeated, using the former apparatus. The mean temperature coefficient of resistance at atmospheric pressure between 0° and 25° for the present sheet material was 0.00591, against the former value for the casting of 0.00564. The present value, being somewhat higher, is presumptive of higher purity. It is to be noticed that both these temperature coefficients are higher than usual for a pure metal. The specific resistance at 0° at atmospheric pressure of the rolled sheet was 5.77×10^{-6}; it was not measured for the former specimen. At 25° the relation between pressure and $-\frac{1}{p}\frac{\Delta R}{R_0}$ for the sheet material was found to be linear, decreasing from 1.68×10^{-6} at atmospheric pressure to 1.52×10^{-6} at 30,000. The corresponding values for the former specimen decreased from 1.77×10^{-6} at atmospheric to 1.46×10^{-6} at 30,000, the drop being not linear, but at a faster rate at the lower pressures.

The present measurements to 100,000 give larger drops of resistance than do the measurements to 30,000, the ratio of the decrements from 20,000 as fiducial increasing from 0.550 at atmospheric pressure to 0.702 at 30,000. This ratio was smoothly extrapolated graphically to unity at 100,000, and the final values listed in Table III were so determined.

Calcium. The material was in lump form, obtained from Mackay in 1938. Since then it has been kept in a glass stoppered bottle under kerosene. There was a heavy crust over the surface, which could be readily scraped off, disclosing bright metal. Specimens for the present measurements were prepared by cutting slivers from the block with a knife and squeezing to flatness between carboloy blocks. Since there was some tendency to crack during this squeezing, different specimens were prepared with different amounts of squeezing or filing, as the case might be, but with no discernible effect on the final result. The general order of dimensions of the specimens was 0.003 inch thick, 0.040 inch wide, and 0.18 inch long.

Four runs were made with two grades of carboloy. These runs appeared satisfactory in all cases, the readings being smooth over the entire range, except for two cases of very small disturbance above 90,000 presumably corresponding to the AgCl transition. Resistance increases under pressure; the four values for change of proportional resistance at 100,000 in terms of resistance at 30,000 as fiducial were 0.791, 0.775, 0.758 and 0.782. In spite, however, of the internally coherent results, the deviation from former measurements[18] to 30,000, was larger than usual. Below 30,000 the presently measured increases of resistance are smaller than before, and the factor in the range 20,000 to 30,000 was 1.712. Furthermore, this factor increases with pressure, being only 1.465 for the interval 0 to 10,000. The problem was how best to apply the correction. The procedure adopted before was clearly out of place, that is, to draw a correction curve approaching unity at the end of the range, for such a correction would pass through a maximum value and would introduce strange curvature into the course of the resistance curve for which there was no experimental plausibility. The simplest correction was adopted, namely to extrapolate linearly the correction factor which reduces the measurements in the former range to

TABLE III

Pressure kg/cm²	R/R_0			
	Beryllium	Calcium	Strontium	Barium
0	1.000	1.000	1.00	1.000
10,000	.984	1.107	1.56	.970
20,000	.969	1.258	2.38	1.008
30,000	.954	1.450	3.47	1.101
40,000	.941	1.686	4.58	1.200
50,000	.929	1.981	3.96[a]	1.340
60,000	.917	2.318	2.37	1.516
70,000	.906	2.710	1.89	1.744
80,000	.896	3.187	1.815	2.032[b] 2.485
90,000	.884	3.753	1.805	2.549
100,000	.876	4.399	1.810	2.618

[a] Maximum of approximately 5.05 at 46,000.
[b] Transition at 80,000.

30,000. The factor so determined rises to 2.66 at 100,000. The results so obtained are given in Table III and Figure 8. In all probability the resistance at the upper part of the range is markedly too high. Whatever the best method of correcting the results it must reproduce the qualitative features of a smoothly rising resistance, with no perceptible episode, and with continually increasing upward curvature.

Previous measurements of the volume of calcium [19] to 100,000 disclosed a polymorphic transition at 64,000 with a volume discontinuity of 1.3 per cent. No trace of this transition was found in the resistance measurements.

Strontium. This was obtained more than 15 years ago from Professor King of Syracuse, and is of demonstrated high purity. It has been kept sealed dry in glass. A thin powdery coat on the surface was rubbed off to give the bright metal. For the measurements it was squeezed between carboloy blocks under Dow Corning fluid 710 to the required thickness of 0.0025 inch. Squeezing under nujol was not successful because of sticking to the platens. Four runs were made with two grades of carboloy. In large features the runs were in agreement. Superficially the relation between pressure and resistance reminds one strongly of caesium. There is a rapid rise of resistance to about 45,000 kg/cm^2, followed by a pronounced drop. Unlike caesium, this drop presently flattens out, and in the last 10,000 kg/cm^2 turns into a slight rise, shown consistently by all four runs. Unlike caesium, the rise to 45,000 is not cusp-like, but is instead a sharp maximum, with definitely reversed curvature in the immediate neighborhood of the maximum on either side. The pressure of the maximum varied in the different runs through a range of about 2,000 kg/cm^2. The abruptness of the drop from the maximum varied from run to run, the major part of the drop being accomplished in from 15,000 to 20,000 kg/cm^2 beyond the maximum. On release of pressure the maximum was again reproduced, but with very great change in the qualitative features, the pressure being displaced from 46,000 to 5,000 and the rise to the maximum being only one third as much as with increasing pressure. This is unlike caesium, where the cusp was repeated with nearly the original height on release of pressure.

Compared with previous measurements,[20] the present change of resistance was less than before, but only about 25 per cent less, a fairly conservative effect. Furthermore, this factor did not vary with pressure, as it did for calcium. The final results were obtained by using a constant correction factor of 1.300, and are shown in Table III and Figure 7.

Previous measurements of the volume of strontium [21] to 100,000 have disclosed two polymorphic transitions: one at 25,000 kg/cm^2 with a volume decrement of 0.9 per cent, and one at 65,000 with volume decrement of

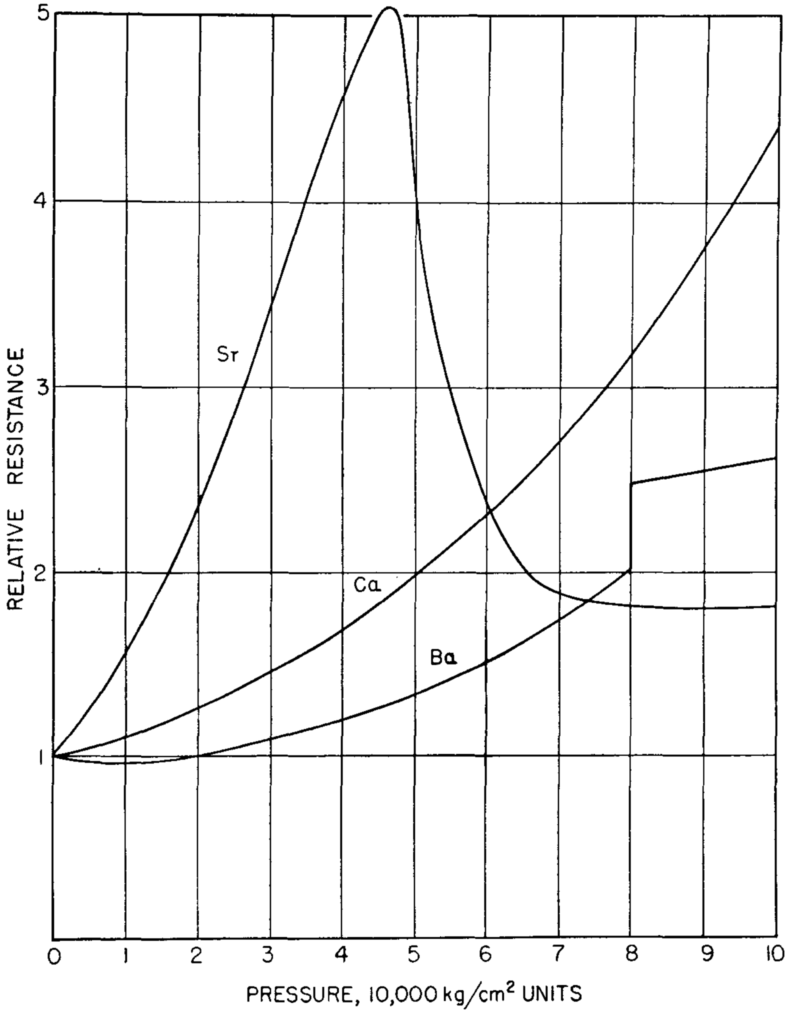


FIGURE 8. The relative resistances of calcium, strontium, and barium to 100,000 kg/cm^2.

0.8 per cent. No effect could be found during the present resistance measurements that could be ascribed to either of these transitions, either with increasing or decreasing pressure. Neither did the former resistance measurements to 30,000 in a true liquid disclose any irregularity that could be ascribed to the transition at 25,000. It is evident that the mechanisms determining volume and resistance must have important points of difference. The maximum of resistance at 46,000 is strikingly large, with nothing corresponding in the volume.

Barium. This was the same material that had been used in the resistance measurements[22] to 30,000; it was originally obtained from Mackay and had been kept sealed under nujol since. It was in the form of extruded wire, and for these measurements was squeezed to a thickness of 0.0025 inch. The chemical activity of this in the air is so high that it was handled in the same vacuum arrangement as the alkali metals. Four runs were made with two grades of carboloy giving qualitatively consistent results. Resistance increases continually from the beginning with rapidly increasing upward curvature, until the process is abruptly terminated at 80,000, followed by a further moderate approximately linear rise from 80,000 to 100,000. The final uprush below 80,000 is so rapid that a first order transition seems the most probable state of affairs, and the final results are given on this basis. However, it must be kept in mind that a second order transition with discontinuity only in the tangent is not definitely ruled out. On release of pressure the rising curve was retraced with unusual fidelity almost in its entirety, except that the pressure of the discontinuity is displaced to 60,000.

Previous volume measurements[23] of barium to 100,000 have disclosed a polymorphic transition at 17,000 with volume discontinuity of 0.6 per cent, and another at 60,000 with volume discontinuity of 1.9 per cent. It is natural to associate the present discontinuity at 80,000 with the former transition at 60,000. Previous measurements of resistance to 30,000 disclosed a small discontinuity in the resistance which probably corresponds to the transition just mentioned at 17,000. The resistance change was so small, however, and showed so much capricious displacement and so great hysteresis between increasing and decreasing pressure that it was entirely smoothed over in the final tabulation of resistance. Distinct evidence of this same transition was found in the present work, and with the same properties of capriciousness and internal viscosity. Definite evidence for it was found twice with increasing pressure in the neighborhood of 40,000, and a third time it appeared to be merged with the approaching transition at higher pressure. The best evidence for it was with decreasing pressure, a definite discontinuity of resistance being observed at approximately the expected pressure on three possible occasions, the fourth observation being made impossible by a blow-out. However, the effect is so small and its location so uncertain that the former course was adopted of smoothing right across the transition in tabulating the final results.

The consistency of the various runs is indicated by the values of proportional resistance at 100,000 in terms of the value at 20,000 as fiducial.

These values were: 2.349, 2.352, 2.057, and 2.067. They were averaged for the final. Compared with previous results to 30,000 the increase of resistance is less than found before, which is the universal direction of discrepancy for metals with a positive coefficient. The best way of correcting to the former values is obscured by the minimum resistance at low pressure. The course was adopted of applying a constant correction factor of 1.323, this being the factor that produces agreement in the interval 20,000 to 30,000. The final values are shown in Table III and Figure 7. This table shows a slight upward curvature between 80,000 and 100,000. This is doubtful; it was demanded by the unadjusted average of the four runs, but two of the runs showed a very slight downward curvature. There was no evidence of disturbance by the transition of AgCl.

Magnesium. This was old single crystal stock which had been used in former measurements. It was rolled to a thickness of 0.002 inch with one intermediate annealing at 0.015 inch thickness. The edges of the rolled strip were frayed out, but the center appeared homogeneous. The specimens were cut with the length perpendicular to the direction of rolling, and widths varying from 0.013 to 0.039 inch were used. Four set-ups were made with two grades of carboloy. The changes of proportional resistance to 100,000 in terms of resistance at 30,000 as fiducial were: 0.0739, 0.0830, 0.0800, and 0.0789, with no apparent correlation with either grade of carboloy or dimensions of the specimen. The four results were averaged.

Compared with previous measurements[24] to 30,000 the present decrements of resistance were 40 per cent smaller. The discrepancy was approximately constant, showing no trend with pressure. The final values were therefore obtained by using a constant correction factor of 1.70. The final results are shown in Table IV. It is to be remembered that magnesium is hexagonal and that therefore anisotropy is a complicating factor. However, previous measurements have shown that the variation of both resistance and pressure coefficient of resistance in different directions is not important for this metal.

Zinc. This was old single crystal stock, from highly purified material which W. A. Zisman had prepared in this laboratory 25 years ago in connection with his measurements of contact potentials.[25] Zinc is markedly anisotropic, the compressibility along the axis being seven times as great as at right angles. The resistance, on the other hand, varies only about 4 per cent with orientation. At any rate, because of the unknown disorienting effect of non-hydrostatic pressure the significance of any results that might be obtained seemed somewhat obscure, and only two runs were made with this material instead of the usual four. One of these was made on a sliver worked out of the original single crystal, the length of the specimen being along the cleavage plane. The other specimen was prepared so as to have as haphazard an orientation as possible by first preparing from the single crystal stock a wire of 0.006 inch diameter by hot extrusion, and then squeezing the wire flat to a thickness of 0.002 inch. The specimen parallel to the cleavage plane showed a drop of proportional resistance at 100,000, calculated from 30,000 as fiducial, of 0.181, while the corresponding change for the extruded and squeezed specimen was 0.151. The average was taken for the final results. In order to compare with previous results,[26] which were obtained on different orientations of the single crystal, it was necessary to average the former results for haphazard orientation.

TABLE IV

Pressure kg/cm^2	R/R_0			R/R_{20}
	Magnesium	Zinc	Cadmium	Mercury
0	1.000	1.000	1.000	
10,000	.953	.937	.927	
20,000	.916	.887	.872	1.000
30,000	.886	.847	.828	.895
40,000	.859	.812	.792	.803[a] .682
50,000	.837	.783	.762	.652
60,000	.817	.756	.736	.627
70,000	.800	.733	.713	.606
80,000	.786	.713	.693	.587
90,000	.776	.695	.675	.570
100,000	.767	.679	.658	.555

[a] Transition

In spite of the fact that the specific resistance in different directions differs by only 4 per cent, the pressure coefficient of resistance in the cleavage plane is more than twice as great as at right angles, a direction of deviation the opposite of what would be expected from the difference of compressibility. When so corrected, the present decrements of resistance are consistently less than the former ones, and furthermore the difference shows a consistent trend with pressure. The final correction was made by assuming a correction linear with pressure. The value of the factor is 1.182 at 30,000 and 1.427 at 100,000. The final results are shown in Table IV. The finally corrected proportional resistance at 100,000 is 0.679 against the value 0.740 which would have been obtained applying no correction whatever.

Cadmium. The material was spectroscopically pure cadmium, prepared by vacuum distillation which I owe to the courtesy of the New Jersey Zinc Company many years ago. For these measurements a small lump was rolled to a thickness of 0.001 inch and used in two widths, 0.010 and 0.020 inch. Four runs were made with two grades of carboloy. These runs gave for the proportional change of resistance at 100,000, in terms of the resistance at 20,000 as fiducial, the values: 0.247, 0.235, 0.248, and 0.257. They were averaged for the final.

Previous measurements[27] have been made on the resistance of cadmium only to 12,000 kg/cm^2, on extruded wire. After those measurements had been made it was found[28] that cadmium exhibits a transition at 5,000 with rather special properties. The change of either volume or resistance at this transition is small, and furthermore is exhibited in measurable magnitude only by highly pure single crystal material. The transition is of that type in which the original crystal is not recovered on release of pressure, but the original single crystal becomes a multicrystal after an excursion back and forth through the transition. The exact significance of any results obtained with cadmium is therefore not plain, and this was the reason for not extending the former measurements to 30,000 along with all the other materials. However, the changes are small due to crystal orientation, and in view of the large changes now of interest in the range to 100,000 it appears that the decision not to measure to 30,000 was a counsel of perfection. Accordingly new measurements have now been made in the 30,000 apparatus with true hydrostatic pressure. The material was wire extruded from the same stock as that used above for the 100,000 measurements. The temperature coefficient between 0° and 25° was 0.00401, and the specific resistance at 0° at atmospheric pressure 6.4×10^{-6}. The results of the new measurements to 30,000 are shown in Table V, given in the same form as for the other metals recently measured over the same range.

Compared with the present measurements in the 100,000 apparatus the proportional changes of resistance in terms of the resistance at 20,000 as fiducial are somewhat larger, the reduction factor being 0.839 at 10,000 and 0.888 at 30,000. The correction factor used in the range 40,000 to 100,000 was obtained by smooth graphical extrapolation of these values to unity at 100,000. The values thus corrected are shown in Table IV.

Mercury. This is liquid below 13,000 kg/cm^2 at room temperature,[29] so that some special method of handling it is required. The difficulties of confining it in immediate contact with the AgCl and getting electrodes into it without leak seemed prohibitive. It was accordingly confined in a very thin flat platinum tube which was inserted into the cell like an ordinary solid specimen. A dummy run with the empty tube gave the contribution to the resistance by the platinum, from which the contribution of the mercury was calculated by the ordinary formula for two resistances in parallel. With the dimensions used, the resistance of the mercury alone varied from 3 to 4 times the total resistance; the relations become more favorable at the higher pressures. The tube was formed from platinum foil specially rolled by Baker and Company to a thickness of 0.0003 inch. The particular piece of foil used for the tube had to be selected to avoid pin holes. It was welded into a flat tube over a form in a stainless steel clamp by heating the whole clamp to a dull red. Excessive temperature must be avoided or there will be sticking to the form or clamp; it was a surprise that so low a temperature would suffice. One end of the tube was sealed flat by the same method. The tube was then tested for soundness with moderate air pressure, sealing it with wax to the source of pressure.

TABLE V

RESISTANCE OF CADMIUM TO 30,000 KG/CM^2

Pressure kg/cm^2	$-\frac{1}{p}\frac{\Delta R}{R_0} \times 10^6$
0	8.55
5,000	7.89
10,000	7.32
15,000	6.84
20,000	6.40
25,000	6.05
30,000	5.72

Filling with mercury was then accomplished by several applications of moderate vacuum, the quantity of mercury controlled by inserting a displacing ramrod, and the open end of the tube sealed sufficiently for the purpose by folding over the open end of the tube on itself several times. The quantity of mercury was then determined by weighing, and the filled tube inserted into the cell in the usual way. The over-all width of the insert was about 0.040 inch and the thickness 0.004 inch.

The specimen thus prepared did not permit acceptable measurements on the liquid, even on the first application of pressure. Even more, after freezing had once taken place, no measurements were possible on release of pressure in the domain of the liquid because of rapid leak, often accompanied by short circuiting between the carboloy blocks by the escaped mercury, which permeated all parts of the assembly, including the pipestone retaining ring. Accordingly, immediately on assembly, pressure was raised rapidly, in a few seconds, to 23,000 kg/cm^2, well above the freezing pressure. Under these conditions freezing took place so rapidly that any escape of liquid was too small to be troublesome. Above 23,000 readings could be made at leisure in the regular way.

Because of the mechanical limitations it is obvious that one will have to be content with less information here than for the usual solid specimen. There are a couple of points of sufficient interest to justify some effort to get even qualitative information. It is known that mercury has a second high pressure form,[30] the transition line with the ordinary form running from −109°C at 10,000 kg/cm^2 to 23°C at 35,000. No measurements of the resistance of the high pressure modification have been previously made, and the present measurements should be capable of giving some information about it. The particular points of qualitative interest are: (a) the direction of the change of resistance at the transition, that is, does it follow the change of volume like most transitions, or is it like the few abnormal transitions, and (b) the sign of the pressure coefficient of the high pressure form.

Two runs were made to 100,000 which appeared fairly satisfactory in the light of internal evidence, and a third run in which the internal evidence indicated that too much of the mercury had escaped on the initial freezing. In addition, a number of abortive attempts at runs were made. The two runs agreed in the main qualitative features. As far as quantitative values go, it would seem that the best that can be done is to average the two runs, since there is no presumption as to which run is better, geometric rearrangement of the mercury within the platinum sheath affording opportunity for the measured effects to be either too large or too small. Both runs agreed in giving a sharp drop of resistance at the transition, which occurred at 40,000 at 26.5° with increasing pressure, or at a mean pressure with increasing and decreasing pressure at 34,000. The formerly determined transition pressure at this temperature was 36,000 kg/cm^2. The third discarded run confirmed the existence of a discontinuity in the same direction at the same pressure. The two good runs did not agree with respect to the amount of drop of resistance; in the one case the resistance of the high pressure form was 80 per cent of the resistance of the low pressure form at the transition, and in the other case, it was 89.1 per cent. The best that can be done is to average these, or 84.5 per cent. Both runs agreed in giving a marked drop in the pressure coefficient at the transition. The average results of the two runs in terms of the resistance at 20,000 taken as fiducial are given in Table IV. Compared with previous results[31] on the low pressure modification between 20,000 and 30,000 the present decrement of resistance is too small by a factor of 0.65. In view of the uncertainties introduced by the transition, no attempt was made to correct for this. The strong probability is that the resistance drops more rapidly at the high pressures than given by the table. The failure of agreement does not necessarily reflect on either the former or the present measurements since there is here a possible effect of anisotropy which would require elaborate study to evaluate.

High pressure mercury thus appears as having a normal negative pressure coefficient of resistance and a numerical value placing it among the softer and more compressible metals, as would be expected.

Aluminum. This was cut from a standard melting point sample of the Bureau of Standards. It was rolled to 0.001 inch thick, stamped to shape, and annealed in vacuum to 400°. Four runs were made, with two grades of carboloy and two widths, 0.010 and 0.020 inch. The proportional change of resistance at 100,000, in terms of the resistance at 30,000 as fiducial, varied from 0.132 to 0.142. The average was used in the final result. Compared with former measurements[32] to 30,000, the decrements of resistance are smaller, and show a consistent trend with pressure, the discrepancy being about 10 per cent at atmospheric pressure and 5 per cent at 50,000 (by extrapolation of the former results). A correction was therefore applied running linearly with pressure to zero at 100,000. The correction is less than usual, and the results should be accordingly more secure. The final results are shown in Table VI.

TABLE VI

Pressure kg/cm²	R/R₀			
	Aluminum	Gallium	Indium	Thallium
0	1.000		1.000	1.000
10,000	.959		.891	.882
20,000	.923		.810	.799
30,000	.892	See	.746	.727
40,000	.865		.694	.665
50,000	.843		.650	.436a
60,000	.824	text	.610	.391
70,000	.808		.578	.352
80,000	.794		.548	.319
90,000	.782		.520	.290
100,000	.770		.493	.265

[a] Transition at 45,000. Resistances .639 and .460

Gallium. The material was recently obtained from Mackay. The melting point at atmospheric pressure is known to be 29.7° and the melting is of the ice type, so that the melting temperature is lowered by pressure. It is therefore liquid at room temperature over the lower end of the pressure range, and it was handled by the same technique as mercury in a platinum sheath. It is known from previous measurements[33] that the melting curve falls to a triple point at 12,050 kg/cm² and 2.4°C with a new high pressure form, the melting point of which rises with pressure. No experimental points have been determined on the melting curve of the high pressure modification, however, and it is known only as a short stretch in the immediate neighborhood of the triple point obtained by thermodynamic calculation. The situation in the neighborhood of the triple point is complicated by the presence of an absolutely unstable form, with properties nearly the same as those of the stable form. Linear extrapolation of the melting curve of the stable form indicated melting at 25° at a pressure of 26,000 kg/cm^2.

The new information to be expected from the present experiment was therefore the approximate freezing pressure of the high pressure form at room temperature, the direction of resistance change on freezing to this form, and the pressure dependence of resistance of the high form up to 100,000.

Gallium is easier to handle than mercury at atmospheric pressure while assembling in the platinum sheath and the cell because it is solid. The nearness of the melting point, however, required that the tools be cooled and all the work of assembly was done on heavy blocks of chilled iron. Gallium has the property that it supports more subcooling than most metals, so that if it is once melted it is difficult to get the solid phase back again. This, combined with the higher freezing pressure at room temperature, made it more difficult to handle under pressure than mercury. Serious attempts were made to get information on the behavior of the liquid phase under pressure, but without success, there always being too much leak. Accordingly the same manipulation was necessary as with mercury, namely rapid freezing by sudden application of pressure. To be on the safe side, 45,000 was chosen as the initial pressure. This did indeed produce freezing, but only with appreciable leak, judging by the internal evidence. The escaped metal might form capricious short circuits between the carboloy blocks.

Four set-ups were made sufficiently promising to justify readings over the range to 100,000, and of these two were free from capricious shorts in the domain of stability of the solid and were used in the final calculations. In addition, several unsuccessful set-ups were made. Calculations were made as for mercury in terms of a dummy run with empty platinum tube. Above 40,000 resistance drops smoothly with increasing pressure. No previous measurements of any sort have been made on gallium above 50,000. The smoothness of the resistance indicates that there are no polymorphic transitions in the new range. In this respect gallium exhibits great poverty of behavior as compared with that other ice-type metal, bismuth, which has six polymorphic modifications below 100,000. Above 40,000 the resistance of solid gallium may be taken with sufficient approximation to be linear in pressure. Any slight curvature is in the normal direction. In terms of the resistance at 40,000 taken as unity the resistance at 100,000 given by the two runs was 0.855 and 0.886, average 0.870.

On release of pressure partial information was obtained on the relations at melting. Melting is accompanied by an increase of resistance, but by how much it would not be safe to estimate because of the disturbing effect of leak. The melting pressure on release could be shut within limits, 4,000 kg/cm^2 apart. The mean pressures of the three usable runs were 38,600, 34,200, and 38,600. There is some doubt about the pressure on release because of the irreversibility in the deformation of the pipestone, but on the other hand there is no overshooting of the melting curve. The pressures just quoted are sufficiently above the extrapolated melting pressure, 26,000, already mentioned, to make it fairly certain that the curvature of the melting curve of the high pressure modification is in the normal direction, that is, concave toward the pressure axis.

Previous measurements of the resistance of solid and liquid gallium[33] to 12,000 had shown that the resistance of the liquid is less than that of the ordinary atmospheric modification. The resistance of both the normal and the high pressure modifications of gallium therefore follows the direction of volume change through a phase change with the liquid. This is the normal direction.

Indium. This was from old Mackay stock, described as 99.9 per cent pure. It was squeezed to a thickness of 0.0035 inch and cut to widths varying from 0.027 to 0.037 inch. Four runs were made with two grades of carboloy. The drop of proportional resistance at 100,000, in terms of the resistance at 30,000 as fiducial was: 0.203, 0.215, 0.225, and 0.209. The simple average was taken. Compared with previous results[34] to 30,000 the present decrements are too small, the expected direction of discrepancy for a compressible metal arising from differential compressibility. The correction factor showed a trend with pressure. For the final values a correction factor was used increasing linearly with pressure from 1.27 at atmospheric pressure to 1.59 at 100,000. The final values of resistance are given in Table VI. The resistance at 100,000 is one half the initial value, and the relation between pressure and resistance shows marked curvature in the normal direction. This is not unexpected in a metal as soft and compressible as indium.

Thallium. This was from my old stock inherited from Professor T. W. Richards who had prepared it for his atomic weight determinations. It was squeezed to 0.003 inch thickness and cut to a width of 0.040 inch. Discoloration of a freshly cut surface in the air was rapid enough to demand filling with the vacuum technique used for the alkali metals. Four runs were made with two grades of carboloy. The proportional decrements of resistance at 100,000, in terms of resistance at 20,000 taken as unity, were: 0.483, 0.421, 0.434, and 0.422. These results were averaged. Thallium is known to have a polymorphic transition[35] at 40,000 kg/cm^2 with volume change of 0.7 per cent. This transition was found in the present measurements at 45,000 with increasing pressure. It is more clean cut than most transitions. It always ran on reaching 45,000 and never at 41,000. It is accompanied by a drop of resistance of 28 per cent, larger than might have been anticipated in view of the smallness of the change of volume. On release of pressure the transition always ran at 28,000 and never at 32,000. It is to be remembered that the absolute pressure on release is uncertain; in the table the discontinuity of resistance is given at 45,000. This does not mean that 45,000 is now regarded as a better value for the transition pressure than the former 40,000 — it is merely that this value was given more directly by the present readings. The value of resistance tabulated at 45,000 for the low pressure modification doubtless applies to that modification in the superpressed condition.

The resistance of thallium had not been previously measured to 30,000. The reason was that thallium is not cubic, and I had hoped to get sometime a single crystal for measurement in the different orientations. However, in view of the large changes of resistance produced by the 100,000 kg/cm^2 of the present work the same remarks apply as to cadmium and special measurements were made of the resistance of thallium to 30,000 in the form of extruded wire and therefore unknown crystal orientation. The average temperature coefficient between 0° and 25° of the specimen measured to 30,000 was 0.00436, and the specific resistance at 0° 1.74×10^{-5}. The new results to 30,000 are shown in Table VII. Thallium has been previously measured[36] to 12,000. The decrement of resistance at 10,000 formerly found was 0.094 against 0.118 now. The former temperature coefficient of resistance was 0.00518.

The decrements of resistance given by the measurements to 100,000 were smaller than those from the 30,000 measurements, as is usually the case for soft highly compressible metals. The ratios did not vary consistently, however, being 0.718, 0.763, and 0.658 at atmospheric, 10,000 and 30,000 respectively, so that the best method of extrapolating the correction is in much doubt. The course adopted was to use a constant correction factor of 0.658 from 30,000 to 100,000, and the results shown in Table VI were so obtained. It must be recognized that the high pressure results are more in doubt than usual. There would seem to be no reasonable doubt, however, of the qualitative results that the resistance drops by a fairly large amount at the 45,000 transition, and that above 45,000 the resistance of the high pressure modification drops with a large coefficient.

Lanthanum. Lanthanum shows highly unusual phenomena in which it is unique. In all, ten runs were made to 100,000 on material from two different sources and with different treatments. Previously the electrical resistance had been measured to 12,000 in 1927[37] on presumably pure material and again in 1951[38] on somewhat less pure material, and the compressibility has been measured[39] to 45,000 and to 100,000. The compression measurements showed a transition at 23,700 kg/cm^2 with fractional volume change of 0.0036. This transition did not appear in the resistance measurements to 30,000, which did, however, show so much hysteresis that it seemed desirable to tabulate the results with both increasing and decreasing pressure.

Of the ten present runs to 100,000, four were made on the same material obtained from Mackay on which the 1951 resistance measurements were made. After these measurements had been completed I was fortunate to obtain some highly purified material through the courtesy of Dr. F. H. Spedding of the Atomic Energy Commission. Two runs to 100,000 with this material gave results qualitatively similar to those with the other material, but there were minor differences between all six of these runs which were greater than to be expected. Qualitatively, the resistance passes through a stage of markedly accelerated decrease between 25,000 and 50,000, above which the rate of drop becomes less again. All these six specimens had been prepared by cutting small pieces from the original massive blocks and cold pressing these pieces between carboloy platens to the desired thickness. The material was therefore in a condition of considerable cold work, the effects of which, in view of experience with other materials and the comparative softness mechanically of lanthanum, were not anticipated to be of importance. At this stage shearing measurements were made on lanthanum in a new carboloy apparatus to 100,000. The shearing measurements showed no transition in the neighborhood of 25,000, and the only feature was an accelerated rise of shearing strength above 80,000. These shearing measurements yielded sheared discs of appropriate

TABLE VII

RESISTANCE OF THALLIUM TO 30,000 KG/CM^2

Pressure kg/cm^2	$-\frac{1}{p}\frac{\Delta R}{R_0} \times 10^5$
0	1.400
5,000	1.276
10,000	1.180
15,000	1.098
20,000	1.025
25,000	0.960
30,000	0.910

dimensions, and the resistance measurements to 100,000 were repeated on a specimen cut from one of these discs. The results were strikingly different from those previously obtained, the accelerated drop of resistance being smeared out over a wide range. It thus appeared that the resistance of lanthanum is unusually sensitive to cold working, and the clue thus afforded was followed up. Three more runs were made to 100,000, the first on a specimen sawed and filed from the original massive block, the second on a strip filed from a wire extruded at dull red heat (it was assumed that the wire would self-anneal after the extruding because of the high temperature), and the third on a specimen similar to the second except that it was annealed in vaccum at 500° for two hours and cooled with the furnace over night.

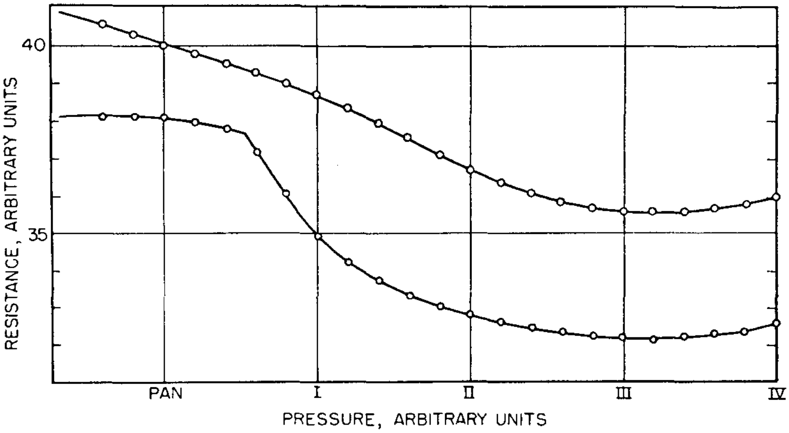


FIGURE 9. The resistances on an arbitrary scale to 100,000 kg/cm^2 of lanthanum in a severely cold-worked condition and annealed. There is no significance in the different origins for the two specimens.

In Figure 9 are shown the directly measured resistances with increasing pressure to 100,000 of the heavily sheared material and of the sawed and filed specimen. The smearing out of the transition or whatever it is for the sheared specimen is striking.

The three runs on the non-squeezed material gave essentially the same results; these three runs were averaged for the final results. Resistance is very nearly constant up to 25,000, in two cases decreasing slightly and in the other passing through a flat maximum. Between 25,000 and 30,000 a drop begins with cusp-like rapidity. The pressure of the cusp varied from 26,000 to 29,000. Any actual discontinuity of resistance corresponding to the previously found transition was too small to establish. The rapid drop of resistance presently flattens out with a point of inflection, and near 90,000 the resistance passes through a flat minimum, shown by all three runs.

The former measurements to 30,000 were now repeated with the new pure material in the form of annealed extruded wire. The average temperature coefficient of resistance at atmospheric pressure between 0° and 25° was 0.00218. This is to be compared with my previous values 0.00218 and 0.00213, the latter for the purer material. There would not seem to be much room for doubt that the temperature coefficient of pure lanthanum is only about one half the value to be expected for most pure metals. The specific resistance at atmospheric pressure at 0° was 6.24×10^{-5}. This is to be compared with the previous values 6.13 and 5.76×10^{-5}, the latter for the purer material. There seems no consistent variation of specific resistance with presumptive purity.

TABLE VIII

RESISTANCE OF THREE RARE EARTH METALS TO 30,000 KG/CM^2

Pressure kg/cm^2	R/R$_0$			
	Lanthanum			
	Increasing	Decreasing	Praseodymium	Neodymium
0	1.000	1.000	1.0000	1.0000
5,000	.991	.983	.9973	.9910
10,000	.983	.956	.9956	.9850
15,000	.975	.927	.9955	.9756
20,000	.965	.916	.9959	.9694
25,000	.935	.910	.9969	.9640
30,000	.894		.9980	.9600

The resistance measurements to 30,000 showed the beginning of the same effects which were found to 100,000. Between 20,000 and 30,000 a rapidly accelerated drop of resistance begins, with no slackening of this drop below 30,000. On reversal of pressure there is great hysteresis, and rapid recovery of resistance does not begin until 15,000. The resistance as a function of pressure with increasing and decreasing pressure to 30,000 is shown in Table VIII. At the higher pressures the results do not agree at all with the 1951 values, the hysteresis being much more pronounced, but the slope at atmospheric pressure, both with increasing and decreasing pressure, agrees rather closely with 1951. It would appear that the delayed internal change which is responsible for the rapid drop of resistance has not begun at atmospheric pressure with increasing pressure, and has been nearly completely wiped out at atmospheric pressure on release.

No plausible scheme presented itself for correcting the results to 100,000 so as to secure agreement with the new results to 30,000. The transition, being spread over a wide range, is sensitive to the width of the range, and the interval from 10,000 to 30,000, which would have to be basic to any correction that might be determined, is in a sensitive part of this range. Accordingly the average of the three runs to 100,000 are presented without correction as the final results in Table IX and Figure 10. It is to be expected that these are uncertain by something of the order of the correction which has been used for other more normal metals of the same compressibility. Qualitatively, a rapidly accelerated drop of resistance begins somewhere between 25,000 and 30,000 (corresponding to the transition previously found in the volume measurements) the drop slowing down and presently reversing in direction. The presumption is that the new modification which gradually appears over a wide range has completely supplanted the original modification in the neighborhood of 90,000. This new modification in the pure state has a smaller resistance than the original, but a positive pressure coefficient of resistance. On release of pressure the reverse transition apparently does not begin to run until the pressure of the cusp has been reached or passed, so that in the entire range from 100,000 to less than 25,000 with decreasing pressure resistance is decreasing, which means a positive pressure coefficient. As already explained, in general the readings with decreasing pressure have not been used, the pressure being uncertain, but all three runs agreed in the features just described and there would seem no room for doubt that it is qualitatively correct. The total drop of resistance on lowering pressure from 100,000 to 25,000 is of the order of 2 or 3 per cent.

TABLE IX

Pressure kg/cm^2	R/R_0			
	Lanthanum	Cerium	Praseodymium	Neodymium
0	1.0000	1.000	1.0000	1.0000
10,000	.9997	.548	.9956	.9850
20,000	.9978	.506	.9959	.9694
30,000	.9759	.487	.9980	.9600
40,000	.903	.477	1.0007	.9550
50,000	.867	.469	.9943	.9527
60,000	.849	.4620	.9312	.9525
70,000	.8398	.4625	.8889	.9525
80,000	.8362	.4711	.8799	.9544
90,000	.8368	.4857	.8819	.9572
100,000	.842	.4817[a]	.8902	.9623

[a] Cusp at 91,000, resistance .4871

Cerium. This material was specially prepared by the Atomic Energy Commission for high purity; it was from the same batch as previously used[40] for measurements to 30,000. The specimens were cut to a width of 0.020 inch from sheet squeezed to a thickness of 0.00027 inch from a nodule cut from the original lump with a knife. There was no evidence of any such effects of mechanical strains as had been shown by lanthanum. In fact, previous experience during the measurements to 30,000 had shown no perceptible effect of complicated annealing procedures.

Four runs were made with qualitatively concordant results. There is in the first place the large drop to approximately half the initial resistance at the known transition at 7,000. The curve continues after the abrupt drop with marked upward curvature. This curvature persists, leading to a minimum near 65,000. The rise with upward curvature now continues until terminated abruptly by an obtuse cusp at 91,000, beyond which resistance drops to 100,000. Expressed in terms of the resistance at 20,000 as unity, the drop in resistance from 20,000 to the minimum varied from 7 to 10 per cent, and the total drop from 20,000 to 100,000 from 3.6 to 6.0 per cent. The four results were averaged. Compared with previous measurements in the range 20,000 to 30,000 the present average resistance decrement is larger by 10 per cent. Present results were corrected to agree with the former ones by applying a correction of 10 per cent to the resistance changes from 20,000 uniformly over the entire pressure range.

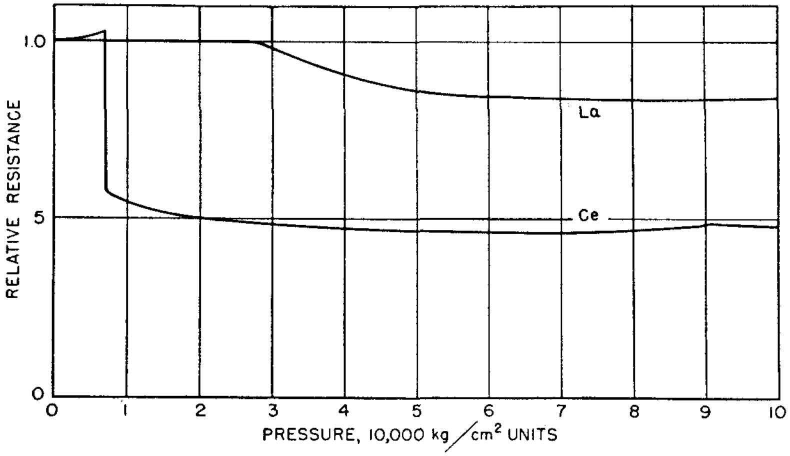


FIGURE 10. The relative resistances to 100,000 kg/cm^2 of lanthanum and cerium.

The final results are shown in Table IX and Figure 9.

Praseodymium. Eight runs were made to 100,000, four on material from Mackay previously measured [41] to 30,000, and four on new material of high purity which I owe to Dr. Spedding of the Atomic Energy Commission. The measurements to 30,000 with true hydrostatic pressure were also repeated with the new material. In large qualitative features praseodymium is strongly reminiscent of lanthanum in that there is a region of accelerated decrease of resistance followed by an upturn. The impurer material shows qualitatively the same effects, except smeared out. There is also some sensitiveness to cold working, although not to nearly as great an extent as shown by lanthanum. The transition starts at a higher pressure and not with so sharp a cusp for the cold worked material. In the final results the average of the last two runs was taken, these being the only runs for which the material was free from all cold work.

The two sets of measurements to 30,000 show a qualitative difference. Formerly, with the less pure material, the resistance decreased over the entire range in a quite normal way. The resistance of the purer material decreases to a minimum near 15,000 and at higher pressures increases. A similar low pressure minimum was shown by one of the two finally retained runs to 100,000, but not by the other, which increased from the start. The average temperature coefficient of resistance at atmospheric pressure between 0° and 25° of the new material was 0.00171 and the specific resistance at atmospheric pressure and 0° 7.37×10^{-5}. For two previous specimens temperature coefficients of 0.00161 and 0.00165 and specific resistances of 7.39 and 6.9×10^{-5} have been obtained. As in the case of lanthanum there can be little doubt that the temperature coefficient is materially less than for most pure metals. The resistance measurements to 30,000 went smoothly, with no trace of hysteresis. The results are given in Table VIII.

The average of the two runs to 100,000 on strain free pure material (one sawed and filed from the block and the other filed from hot extruded wire) are given in Table IX. In this table the results for the first 30,000 kg/cm² were lifted directly from the new measurements over this range. Between 20,000 and 30,000 the average decrement of resistance from the 100,000 measurements was 0.0019 against 0.0021 from the 30,000 measurements. The agreement is unusually good and may be taken to justify the direct use of the 100,000 values at higher pressures without attempt at correction. The drop of resistance does not begin with quite cusp-like abruptness, but there is perceptible rounding. If the curve above and below is extrapolated to a cusp, the pressure of the cusp, that is the pressure at which the internal change begins, has the identical value for the two runs, 51,200 kg/cm². The fact that the run to 30,000 gave no hysteresis would indicate that no trace of this change has begun at pressures below

30,000. Compared with strain-free material, the two runs on presumably strained material differed in that the onset of the transition was displaced to a pressure about 7,000 kg/cm^2 higher. At the top, 100,000, the changes of resistance of both strained and strain-free material were practically identical. The nature of the new form that emerges after pressure passes through the transition zone is similar to the corresponding form of lanthanum in that its resistance increases under pressure. With decrease of pressure from 100,000 resistance decreases continually at a somewhat decreasing rate (that is, the curve of resistance against pressure is concave upward as is normal) until a pressure of 25,000 is reached, when the reverse transition begins and proceeds rapidly. It is to be noted that unlike lanthanum, the volume measurements[42] had given no hint of a transition in the neighborhood of the cusp. It is to be remarked, however, that the cusp for lanthanum being at a lower pressure the volume measurements were made with a considerably more sensitive apparatus than was used for praseodymium.

Neodymium. The history of the measurements on this is much the same as that of praseodymium. Eight runs were made to 100,000, four on material from Mackay, the same as used in recently published results[43] to 30,000 and four on high purity material which I again owe to the courtesy of Dr. Spedding of the Atomic Energy Commission. In addition, the new material was measured at 30,000. In general the results are not like either lanthanum or praseodymium, there being no evidence of internal change, but the effect of pressure on resistance is unusually small.

The results of the new measurements over the 30,000 range are given in Table VIII. There is definite hysteresis, which is averaged out in the table. The average temperature coefficient between 0° and 25° was 0.00164, and the specific resistance at atmospheric pressure at 0° 7.18×10^{-5}. These figures are to be compared with temperature coefficients of 0.00080 for the material measured many years ago[37] and 0.00127 for the recently measured material, the specific resistance of which was 8.05×10^{-5}. It is probable from the wider spread of the figures compared with the corresponding figures for lanthanum and praseodymium that impurity plays a more important part in the behavior of neodymium than of the two others. Again we have a rare earth metal with temperature coefficient markedly less than that of most pure metals. Neodymium is mechanically harder than the two others and much more subject to splintering when cold pressed. It is probable that this effect was responsible for the wide divergence between the results with the two grades of material when measured to 100,000. Three runs on the impurer material, formed by cold pressing, gave large decreases of resistance to 100,000, running to over 20 per cent. A fourth specimen filed from the hot extruded wire gave a drop of only 3 per cent. The difference was ascribed to the closing of cracks in the cold pressed specimens. After these runs, four runs were made to 100,000 on the pure Atomic Energy Commission material, the specimens having been treated as had those of lanthanum and praseodymium (sawing and filing and hot extrusion) to avoid the effects of cold work. The runs agreed in giving much smaller effects than the other material. Three of the four gave a definite minimum and the fourth was apparently approaching a flat minimum. In view of the smallness of the effect the four runs were averaged. Compared with the runs to 30,000 the change of resistance between 20,000 and 30,000 was only one third as much. Since the whole change of resistance of this material is within the possible limits due to the distortion of the carboloy, the only course seemed to be to correct the average results to 100,000 by multiplying the changes of resistance by the constant correction factor 3, and the results so obtained are given in Table IX. There is evidently much doubt with regard to the absolute values of the change of resistance, but I think the qualitative feature of a minimum resistance may be accepted.

Titanium. The material was from the same length of wire as used in recent measurements of resistance to 30,000. It was prepared by the Remington Arms Company under license from Dupont. For these measurements it was rolled to a thickness of 0.003 from 0.0167 inch wire. Previous experience had shown no effect of annealing, and the specimens were not further annealed after rolling. The width was about 0.035 inch.

Four runs were made with two grades of carboloy. The decrements of resistance from 30,000 to 100,000, taken in terms of the resistance at 30,000 as fiducial, varied from 0.050 to 0.058. The average was used. The present decrements of resistance are larger than before[44] in the range up to 30,000 and the ratios show an approximately linear trend with pressure. The present results were corrected to agree with the former ones to 30,000 by a correction factor rising linearly with pressure from 0.74 at atmospheric pressure to 1.00 at 100,000. The final results are shown in Table X. There is no special feature.

Zirconium. Two batches of material were used and two runs were made to 100,000 on each. The first two runs were made on rolled strip obtained from Mackay. The resistance decreases with normal upward curvature to the vicinity of 70,000, where the curve begins to turn over and continues with increasing downward curvature to 100,000. The second set was made with material obtained from the Philips works in Eindhoven a number of years ago which had been used in previous measurements of compressibility and pressure effect on resistance.[45] It had been prepared by vacuum distillation of one of the salts, and was presumably of high purity. The behavior of this purer material was only qualitatively reminiscent of that of the impurer. Resistance drops with normal upward curvature to nearly 80,000, where a precipitous drop begins, which is terminated before 100,000 by the curve turning and resuming approximately its original course. It would seem that this material experiences a polymorphic transition near 80,000, the effect of which was largely obscured in the impurer sample. This probably means that the impurity dissolves in both the low and high pressure lattices, something which does not always occur. Previous measurements of volume to 100,000 disclosed no transition.

TABLE X

Pressure kg/cm^2	R/R$_0$		
	Titanium	Zirconium	Thorium
0	1.000	1.0000	1.000
10,000	.989	.9977	.966
20,000	.979	.9954	.937
30,000	.970	.9933	.912
40,000	.961	9914	.892
50,000	.953	.9894	.876
60,000	.945	.9874	.862
70,000	.938	.9856	.850
80,000	.931	.9836	.840
90,000	.924	[a]	.831
100,000	.916		.821

[a] Transition above 80,000, with 16–17% drop of resistance

The absolute value of the pressure effect for this substance is unusually small; the present uncorrected measurements give a drop of resistance in the first 30,000 kg/cm^2 of less than 2 per cent and the former measurements of less than 1 per cent. The ratios of these small changes were plotted against pressure and a straight line drawn, giving a correction factor running from about 0.4 at atmospheric pressure to about 0.8 at 80,000. The values so corrected are shown in Table X. Considerable percentage error may be expected in the fractional changes of resistance. No attempt was made to give exact values beyond the transition or even at the transition itself. Approximate figures for the transition are indicated in the table. It is probable that the pressure coefficient of the high pressure modification is also negative, but no numerical estimate is justified.

Thorium. This was high purity material prepared by the Atomic Energy Commission, which I owe to the courtesy of Professor Bainbridge, who had obtained it from Dr. Spedding. It was in sheet form, 0.0009 inch thick and was used in a width of 0.040 inch. Four runs were made with two grades of carboloy. The individual runs showed rather more variation than usual, the values for the decrease of resistance at 100,000 in terms of resistance at 30,000 as unity being: 0.125, 0.093, 0.084, and 0.101. The average was used.

The resistance of thorium has been previously measured[46] only to 12,000 on presumably less pure material. Accordingly new measurements were now made to 30,000. The specimen was a narrow strip cut from the same sheet as that of the 100,000 measurements; the four leads were spot welded in position. The average temperature coefficient at atmospheric pressure between 0° and 25° was 0.00275, and the specific resistance at 0° 2.67×10^{-5}. The temperature coefficient of the former material was 0.00239. The temperature coefficient indicates higher presumptive purity in the present specimen. The resistance measurements to 30,000 ran smoothly, with no hysteresis. The initial pressure coefficient is about 23 per cent greater than on the former material to 12,000. Within experimental error resistance is a second degree function of pressure, that is, $-\frac{1}{p}\frac{\Delta R}{R_0}$ is linear with pressure, decreasing from 3.610×10^{-6} at atmospheric pressure to 2.926×10^{-6} at 30,000, making the net decrease of resistance at 30,000 8.78 per cent.

The average of the four runs to 100,000 gave changes of resistance in the first 30,000 agreeing with the above within 2 per cent or better. The directly measured values were therefore used without correction, and these are the final values shown in Table X.

Carbon (*Graphite*). This was single crystal Ceylon graphite which I owed to the courtesy of Sir K. Krishnan a number of years ago. Specimens were prepared from cleavage sheets with length in the cleavage face of thickness 0.004 inch, width 0.080 inch, and the usual length 0.18 inch. There was considerable variation in the apparent perfection of the material. The most favorable appearing piece was selected and two specimens cut from it as closely contiguous as possible. The over-all results on these two specimens were nearly the same, the proportional decrements of resistance at 100,000 in terms of the initial resistance at atmospheric pressure as unity being 0.229 and 0.241. In spite of this gross agreement there were marked qualitative differences, the one specimen showing a shallow minimum at 14,000 followed by a flat maximum at 28,000, while the other showed only a minor interruption in its smooth course in this whole neighborhood. In view of the nature of the material it did not seem worth while to accumulate measurements, and only two runs were made. The average of the two is shown in Table XI. The abnormal downward curvature at high pressures was shown by both specimens, as was also the high initial rate of drop up to 20,000. The near minimum and maximum between 20,000 and 30,000 was contributed by only one specimen. There are no previous measurements of the effect of pressure for comparison, the only previous measurements[47] having been made on Acheson graphite, which is not comparable because of its great porosity, to mention only one reason. Paradoxically the resistance of Acheson graphite was found to increase by a few per cent up to 12,000.

Some indication of the character of the present graphite may be obtained from its specific resistance. Rough values calculated for the specific resistances of the two specimens from the initial resistance and the dimensions were 116 and 109 × 10^{-6} ohm cm at room temperature. These figures may be too high by a few per cent because of electrode resistance, for which no correction was made. These specific resistances are to be compared with the value 39 × 10^{-6} given in International Critical Tables for "single crystals" material, orientation not specified.

TABLE XI

Pressure kg/cm^2	R/R$_0$				
	Ceylon Graphite	Silicon	Germanium	Tin	Lead
0	1.000			1.000	1.000
10,000	.949			.910	.873
20,000	.911	See	See	.837	.779
30,000	.904			.775	.704
40,000	.893	Text	Text	.724	.647
50,000	.870			.683	.603
60,000	.852			.647	.570
70,000	.833			618	.543
80,000	.813			592	.521
90,000	.790			.569	.502
100,000	.765			.548	.487

Silicon. This material is a non-metal, is mechanically very brittle, and electrically is a semi-conductor. The resistance is supposed to be sensitive to "dislocations" mechanically produced. It would therefore be expected that any resistance measurements under pressure would be affected by the non-hydrostatic character of the stress more importantly than the metals of this paper. This did indeed prove to be the case; in fact the runs proved to be so difficult to interpret that only a few runs were made. There were, however, certain qualitative features that probably are of significance, and the results will be described sufficiently to indicate what these were.

The same high purity material was used, originally obtained from the

Bell Telephone Laboratories, that was used in recently published measurements to 30,000 under true hydrostatic pressure.[48]

The high specific resistance of this material suggests a modification of the method of mounting the specimen. Considerable effort was made to use the specimen in the form of a single button at the center of the AgCl cell, reaching through the cell into contact with the carboloy blocks on either side. It proved necessary to use amalgamated copper electrodes to regularize the contact, and when this was done there was always short circuiting between the blocks by way of copper squeezed into the crack between the specimen and AgCl. It was accordingly necessary to return to the use of a bar-shaped specimen in the AgCl sandwich. This meant initial resistances of the order of a few thousand ohms, and this meant a complete rearrangement of the electrical circuits. The details need not be given.

One run was made with p-type silicon. Formerly, up to 30,000, it was found that the resistance increased through a maximum and was decreasing over the last 10,000 kg/cm ^{2}. On release of pressure there was hysteresis of two thirds the maximum effect. The entire range of resistance change was confined to 20 per cent. The new measurements gave irregular results, as usual, at low pressure, but from 10,000 to 36,000 a nearly linear decrease of specific resistance from 4.3 to 3.5 ohm cm. At 36,000 there was an abrupt drop to 2.3 ohm cm, followed by a further gradual drop to a minimum of 1.9 at 60,000, followed by a smooth rise to 3.3 at 100,000. On release the deep minimum was confirmed as also the rise at lower pressures with an abruptness suggesting some kind of transition.

Two runs were made with n-type silicon. Previous measurements to 30,000 had given an approximately linear decrease of resistance to about 50 per cent initial, with moderate hysteresis on release. In the present measurements both specimens gave an initial rapid drop of resistance, but this continued to only 10,000 kg/cm ^{2}. Here a rise began by a factor of several fold for both specimens, but in the range 10,000 to 30,000 the details exhibited by the two specimens differed greatly. From 30,000 up to 100,000 both specimens agreed in a smooth decrease, with only minor interruptions of regularity, to a value at 100,000 approximately one third that at 30,000. On release, the episode of rapid rise from the high pressure side was confirmed. In spite of the rough qualitative agreement of the runs with the two specimens, which were cut from contiguous parts of the same piece, there was no agreement at all on the absolute values of specific resistance. Under 100,000 kg/cm ^{2} the specific resistance of one specimen was 0.11 ohm cm and of the other 7.6. The initial resistance at atmospheric pressure was 3.9 ohm cm. Strains and mechanical imperfections must be playing a preponderant role.

Germanium. This is a brittle semi-conductor and the same remarks apply as to silicon. Two runs were made, one on n-type and one on p-type. The method of mounting the specimens was the same as for all the other substances, as the inner layer in the AgCl sandwich. The electrical connections were the same as for silicon, demanded by the high resistance. The identical material was used as in recently published results[49] to 30,000. Fairly reproducible results were obtained with germanium; it was recognized by the Bell Telephone Laboratories that their germanium was more satisfactory than their (or anyone else's) silicon.

The previous measurements of n-type germanium up to 30,000 gave a moderate rise of resistance, by about 25 per cent up to 15,000, where there was a rapid upturn in the rate of rise, which at 30,000 amounted to a four fold increase of initial resistance. The initial specific resistance of the specimen of n-type germanium measured to 100,000, obtained by extrapolation to atmospheric pressure, was 18.7 ohm cm, in practically perfect agreement with the value 19 of the Bell Laboratory. The initial effect of pressure up to 7,000 was a decrease of resistance of 19 per cent, followed by a rapid upswing to a sharp maximum at 50,000 at double the initial resistance, followed by a smooth drop to 30 per cent more, at 100,000, than the initial resistance at atmospheric pressure. It would appear that these new results are not inconsistent with the previous ones to 30,000 if one takes seriously the suggestion that dislocations produced by strains in this material result in a decrease of resistance. The initial drop to 7,000 would then be ascribed to this effect. At 7,000 the natural rise takes over, but the dislocation effect persists and accounts for the rise to only double initial resistance instead of to 4.5 fold and more. The new feature contributed by the present measurements and which would seem to be qualitatively real, is the maximum at 50,000 and the rapid drop of resistance beyond it.

Previous measurements on p-type germanium to 30,000 gave an approximately linear drop of resistance of 6 per cent, with minor hysteresis which tended to vanish on repetition of the pressure cycle. The present measurements also gave a smooth decrease, except for an initial disturbance of no probable significance, amounting to 15 per cent at 30,000, and continuing to 82,000, where the drop is by 33 per cent. At 82,000 there is a minimum, beyond which resistance rises by 6.5 per cent to 100,000. The specific resistance of this specimen at atmospheric pressure was approximately 10 ohm cm, against the value 3.35 given by the Bell Telephone Laboratory.

Tin. The material was old Kahlbaum "K" stock. It was squeezed to a thickness of 0.001 inch and used in two widths, 0.010 and 0.020 inch. Four runs were made with two grades of carboloy. The proportional changes of resistance at 100,000 in terms of resistance at 30,000 as fiducial were: 0.246, 0.246, 0.204 and 0.171. The two latter and lower values were obtained with grade 883 carboloy and the two others with grade 905. It does not appear whether the difference is significant — probably not. The four results were simply averaged. Compared with previous measurements[50] to 30,000, extrapolated to 50,000, the present decrements of resistance are too small. The ratio is nearly independent of pressure, and in making correction the present resistance changes were multiplied by the constant correction factor 1.35. The final result is given in Table XI. Resistance drops smoothly by a large amount; a large effect would be expected in so soft and compressible a metal.

Lead. This was of 99.9999 per cent purity, the same as has been used in a number of my previous measurements. It was squeezed to 0.001 inch thickness and used in widths of 0.010 and 0.020 inch. Four runs were made with two grades of carboloy. The proportional change of resistance at 100,000 in terms of the resistance at 30,000 as fiducial, given by the four runs was: 0.314, 0.299, 0.318, and 0.311. The results were averaged. Compared with previous measurements[51] to 30,000 the decrements of resistance now obtained are smaller than before. In the range of actual measurement, that is up to 30,000, the ratio between present and former decrements decreases approximately linearly with pressure and heads for the value 1 and 100,000. Accordingly a correction factor was used dropping linearly with pressure from 1.25 at atmospheric pressure to 1.00 at 100,000. It is to be noticed that if the former results are extrapolated beyond 30,000 by assuming constant second differences there will be no agreement, the extrapolated results indicating a minimum in the neighborhood of 60,000 for which there is no experimental evidence. This indicates the caution necessary in inferring the existence of any unusual feature by extrapolation. The final results are shown in Table XI. At 100,000 the resistance drops to 0.49 initial, an effect a few per cent greater than for tin.

Vanadium. It is only recently that this material has been available in pure form. I owe my specimens to the courtesy of the Research Laboratory of the General Electric Company at Schenectady. Material was supplied which had been prepared by two different methods. One was "calcium reduced vanadium." This was furnished in the form of a smooth rolled strip 0.004 inch thick and 0.5 inch wide. The analysis in weight per cent was: C $\sim$ 0.20, Fe $<$ 0.01, Si $<$ 0.01, Ca $<$ 0.01, O_2 0.03 to 0.05, $H_2 \sim 0.002$, $N_2 \sim 0.01$ to 0.03. The second material was "hot wire deposited vanadium". It evidently had been stripped from the hot wire and rolled, but the rolling left it in very irregular shape with longitudinal cracks and curls and badly frayed edges. The central portion appeared homogeneous. The analysis of this was: Fe 0.01 to 0.015, Al $<$ 0.005, Si 0.01, Cr $<$ 0.005, Mn $<$ 0.005, Mo n.d., W n.d., Ni n.d., Ca tr, Mg tr, Cu 0.027 to 0.050, H_2 0.0004 to 0.0013, $N_2 <$ 0.004 to 0.008. Two runs were made, one on each grade, with a softer grade of carboloy, #779, than used in most of this work. Resistance decreased to 100,000 smoothly, with no episode of any kind. The proportional change of resistance at 100,000, in terms of the resistance at 20,000 as fiducial, was 0.1064 for the hot wire deposited material and 0.0799 for the calcium reduced.

A strip cut from the calcium reduced material was measured to 30,000. Its average temperature coefficient between 0° and 25° was 0.00366, and the specific resistance at atmospheric pressure at 0° 2.09×10^{-5}. The relation between pressure and resistance is sensibly of the second degree in pressure up to 30,000. The value of $-\frac{1}{p}\frac{\Delta R}{R_0}$ drops linearly with pressure from 1.660×10^{-6} at atmospheric pressure to 1.524 at 30,000. Below 15,000 there are slight deviations from the regularity of the relation, with some hysteresis, which, however, is probably too small to be significant.

TABLE XII

Pressure kg/cm^2	R/R$_0$ Vanadium	R/R$_0$ Columbium	R/R$_0$ Tantalum
0	1.000	1.000	1.000
10,000	.984	.986	.984
20,000	.969	.973	.968
30,000	.954	.961	.954
40,000	.941	.950	.941
50,000	.929	.938	.929
60,000	.916	.928	.918
70,000	.906	.918	.908
80,000	.895	.909	.898
90,000	.885	.901	.890
100,000	.878	.894	.882

The resistance decrements given by the mean of the two runs to 100,000 were larger than those given by the measurements to 30,000, the ratio varying from 0.68 at atmospheric pressure to 0.82 at 30,000. This factor was extrapolated to the value unity at 100,000 by free hand graphical construction, and the values obtained with the correction so determined are given as the final values in Table XII. Vanadium seems normal in every way in its behavior to 100,000.

Columbium. This was laboratory stock from the Fansteel Company. It was in sheet form 0.0035 inch thick and was rolled to 0.0012 inch thick and annealed to red heat in vacuum. Four runs were made with two grades of carboloy, and with widths varying from 0.017 to 0.022 inch. The proportional changes of resistance at 100,000 in terms of resistance at 30,000 as fiducial varied from 0.067 to 0.082. The average was used. Compared with previous measurements[52] to 30,000 the present changes of resistance are too large. The reduction factor varies linearly from 0.68 at atmospheric pressure to 0.79 at 50,000 (by extrapolation). The present results were corrected by a factor varying linearly over the entire range from 0.69 at atmospheric to 0.93 at 100,000. The results are normal in every way, smooth decrease with upward curvature.

Tantalum. This was from the same laboratory stock from the same source as the columbium. It was used in the original sheet thickness of 0.003 inch and in a width of 0.035 inch. Four runs were made with two grades of carboloy. The proportional change of resistance at 100,000, in terms of the resistance at 30,000 as fiducial, varied from 0.087 to 0.114. The average was used. Compared with previous measurements[53] to 30,000 the present results give too large a decrease of resistance. The ratio increases with pressure, but the ratio is not as smooth as usual and the final results are in more doubt. Final correction was made by using a correction factor increasing linearly with pressure from 0.55 at atmospheric pressure to 0.77 at 100,000. The final results are shown in Table XII.

The behavior is normal in every way.

Arsenic. The material was old highly purified single crystal stock. It is brittle and fragile and difficult to handle. The specimens were prepared in two radically different ways. The first method was by splitting off from the cleavage plane of a single crystal with a razor blade a thin slab, reducing this in thickness to about 0.005 inch by squeezing cold between carboloy platens, and then worrying out of the flake thus formed a very roughly shaped specimen. This latter was difficult because of the many cracks produced by the flattening process and the extreme fragility of what hung together. Finally, however, two specimens were produced which appeared to be sound. Runs with these two specimens gave results entirely different qualitatively. The first gave a smooth decrease with normal upward curvature to about 70,000, where there was a slight inflection followed by decrease to 100,000 with increasing downward curvature. The second gave a decrease to a minimum at 40,000, rise to a maximum at 80,000, and drop with abnormal downward curvature to 100,000. Previous measurements[54] to 12,000, the previous maximum, had prepared me for unusual and nonreproducible effects with this material, but not for variation as great as this. Accordingly, other specimens were prepared by hot extrusion from a steel die. As far as I know this material has not been extruded before. The temperature of the extrusion is apparently rather critical; if temperature is too low arsenic will not extrude at all and if too high will completely vaporize away as it emerges from the die. The method used was empirical, heating the die with a gas torch while pressure was exerted on the extruding piston and taking the wire when it came. Enough wire 0.0055 inch in diameter was secured in this way to allow three different set-ups, but not more than enough, the wire completely vaporizing away at the last. The specimens were prepared by laying five lengths of the wire side by side in a trough preformed in the AgCl. The depth of the trough was slightly less than the diameter of the wire, so that when the sandwich was subjected to its final consolidation the AgCl flowed around the wire. In spite of the extreme brittleness of this material examination under the microscope disclosed no initial fractures, and the behavior of electrical resistance bore this out. Three runs were made with the extruded wire, with two grades of carboloy. The three runs were in qualitative agreement and agreed with one of the runs with the specimens prepared by the other method, namely decrease to a minimum, rise to a maximum, and drop. It is to be presumed that the orientation in the extruded wire is more or less haphazard, so that the three runs with minimum and maximum may be assumed to be a sort of average for different directions in the crystal. The similar behavior of the specimen prepared by squeezing from the cleavage flake may be ascribed to the disorientation produced by the process of squeezing. But since one specimen from the cleavage flake showed a radically different behavior, the presumption is that there is one direction in the crystal with that sort of behavior and qualitatively different from the mean.

There seems no basis for comparison with previous results, which exhibited various non-reproducible breaks in direction and two-fold variation in numerical magnitudes. The rough general average of previous results to 12,000 was a decreasing resistance with abnormal downward curvature. The best that it seems possible to do with present results is to average the four qualitatively similar minimum-maximum results, and to reproduce for itself without correction the one qualitatively different. The proportional decrease of resistance at 100,000 of the resistance at atmospheric pressure as fiducial of the four minimum-maximum runs varied from 0.052 to 0.089, average 0.072. This is much less than for the other specimen, with a corresponding decrease of resistance of 0.302. The numerical results are given in Table XIII and Figure 11.

Antimony. The material was old single crystal stock, and presumably of high purity, having been prepared from Kahlbaum "K" material. It is very brittle and requires special manipulation. One run was made on a sliver of suitable dimensions obtained by cleaving off flakes with a razor blade from the cleavage face of a massive crystal. However, the production of this specimen proved to be a matter of good luck, and I was not able to make another in spite of destroying much single crystal material. Accordingly, the method used with arsenic was adopted, namely cleaving off as thin a flake as possible, squeezing this to the required thickness, here about 0.006 inch, between carboloy platens, and then worrying out of this flake a roughly shaped specimen. Runs made on two specimens fashioned in this way agree qualitatively with the results on the single crystal flake, namely rise to a maximum followed by drop and reversal of curvature to normal in the upper 20,000 kg/cm². This eventual reversal of curvature is of course inevitable, for otherwise resistance would become negative at sufficiently high pressure.

TABLE XIII

	R/R_0			
	Arsenic			
Pressure kg/cm²	Haphazard orientation	Mostly in cleavage plane	Antimony	Bismuth
0	1.000	1.000	1.000	1.000
10,000	.959	.933	1.060	
20,000	.925	.882	1.104	(a)
				(b)
30,000	.901	.844	1.103	.604
40,000	.891	.814	1.078	.567
50,000	.894	.789	1.021	.546
60,000	.907	.767	.938	.528
70,000	.925	.750	.837	.517
80,000	.937	.734	.756	.498
90,000	.938	.718	.674	.485
100,000	.928	.698	.605	.474

[a] Transition I–II at 25,650. Resistances 1.514 and .239
[b] Transition II–III at 27,080. Resistances .236 and .617

The three specimens thus measured may be presumed, because of the method of preparation, to have a strongly preponderating orientation of the basal plane in the length. Previous measurements[55] to 30,000 had shown strong orientation effects. Thus when the axis is at 87° to the length (cleavage plane 3° to the length) resistance increases by about 5 per cent to a maximum in the neighborhood of 10,000, and from here decreases with abnormal downward curvature to 0.95 at 30,000. When the axis is at 41° to the length, however, resistance increases with abnormal downward curvature over the entire 30,000 with an extrapolated maximum at 31,250. It was to be expected therefore that with a greater preponderance of lower orientations the maximum would be accentuated. A run was accordingly made on a specimen prepared by grinding to suitable thickness a wire prepared by hot extrusion from commercial material some 30 years ago. The results were not as expected at all, but from the start resistance decreased, at first slowly, then more rapidly, and finally with reversal of curvature. The only explanation of this seemed to be inferior chemical purity in the commercial material. Accordingly wire was freshly prepared by hot extrusion from the high purity single crystal stock. Extrusion was more difficult than with the commercial material and only short lengths of suitably smooth wire of 0.0045 inch diameter was obtained (most of it was curled). This was mounted like arsenic in three parallel lengths and a run made to 100,000. The result was now as expected, an enhanced maximum as compared with the three

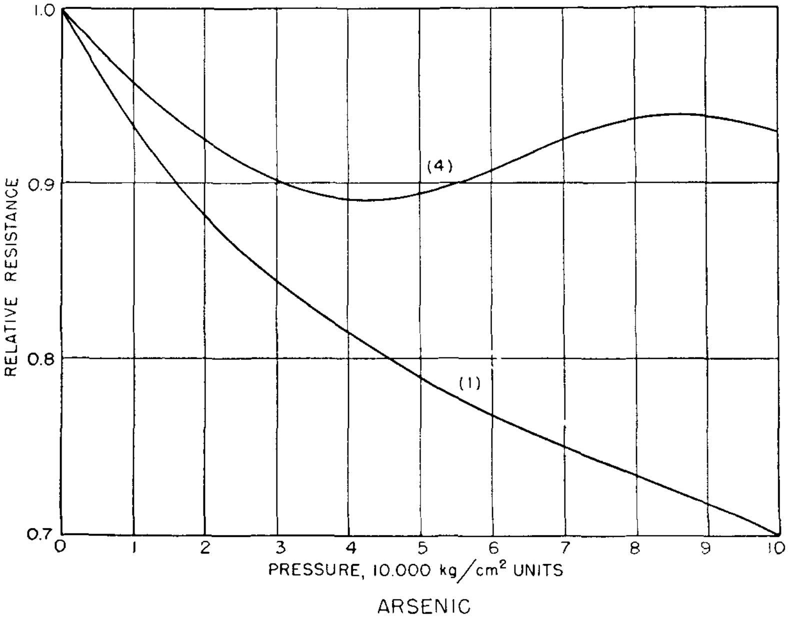


FIGURE 11. The relative resistance to 100,000 kg/cm^2 of arsenic. Curve (4) is the average of four similar specimens and curve (1) the curve for a single exceptional specimen.

others, and a displacement of the pressure of the maximum toward higher pressures.

In view of the strong orientation effects no good way of adjusting these values to agreement with the former ones at 30,000 presents itself, and the four runs were simply averaged. It would seem that this average corresponds to an angle between the axis and the length of between 70° and 80°. The results are shown in Table XIII and Figure 12. From a study of the variation in the pressure of the maximum it would seem probable that if any adjustment were made of the tabulated results it should be in the usual direction of a stretching out of the changes of resistance so that both increases and decreases would become larger. However, the best way of doing this did not appear.

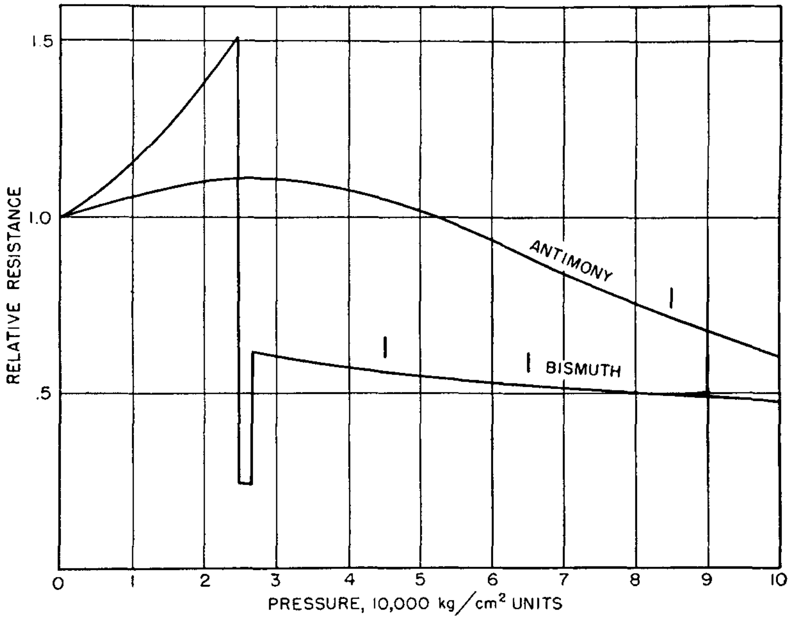


FIGURE 12. The relative resistances to 100,000 kg/cm^2 of antimony and bismuth. The vertical dashes show the location of polymorphic transitions as disclosed by the measurements of volume.

None of the specimens showed any trace of the polymorphic transition found previously at 85,000 on measurement of compressibility.

Bismuth. The material was from my highly purified electrolytic stock, cast into single crystal form after electrolytic deposition. Specimens were prepared by several methods. Two specimens were cut from a sheet 0.002 inch thick made by allowing a drop of molten metal to solidify between carboloy platens clamped to within 0.002 inch of contact by suitable shims. Two specimens were cut from a thin film made by allowing a molten drop to fall from a suitable height onto a cold iron plate, spattering itself out into a thin film, which was further squeezed cold between carboloy platens to 0.002 inch thick. In order to accentuate any possible orientation effects a fifth specimen was made by cleavage with a sharp knife from the basal plane of a single crystal. Bismuth in thin section is fairly flexible and none of the mechanical difficulties were found that had made manipulation of arsenic and antimony so difficult.

Bismuth is known to have five polymorphic transitions[56] up ot 100,000 (six polymorphic forms), including two with large volume change near 25,000. On passing through these transitions and back the original orientation is not recovered, but the original single crystal is converted into a multi-crystal. Orientation effects would therefore not be expected in the present work, and none were found. In fact, the structure of the high pressure modifications is not known; if it should happen to be cubic then no orientation effects can occur in principle under any conditions with these modifications. The first transition, I—II, is known from previous work[57] to be accompanied by a drop of resistance by the large factor 6.34. The precise value of this factor is not of definite significance because of the unknown orientation effects. The second transition, II—III, is known from previous work to be accompanied by a rise of resistance by a factor 2.62, the rise being against the direction of volume change and therefore abnormal. In the present measurements these figures were qualitatively confirmed; the resistance drops at the I—II transition by an amount varying in the five runs from 2.5 to 4.1, and in four out of the five runs rises at the II—III transition by a factor varying from 1.25 to 1.79. The fifth run gave a factor 0.8 (drop).

The presence of two large transitions near 25,000 nullifies any attempt to find a satisfactory basis for reducing the present measurements to agreement with the former ones below 30,000. However, the evidence is that the agreement is unusually close in the range below the I—II transition, the present increase of proportional resistance between atmospheric pressure and 25,090 varying from 1.39 to 1.50 against a former value of 1.52. Accordingly the present values were averaged, without further correction, in the range 40,000 to 100,000, choosing the fiducial resistance at 40,000 so as to agree with the former value extrapolated from 30,000 to 40,000. In the range above 40,000 the five runs were in rather better agreement than usual, the extreme values for proportional change of resistance at 100,000, in terms of that at 40,000 as fiducial, being 0.160 and 0.170. All five runs in the range above 40,000 agreed in giving almost no perceptible effect of the other three known transitions. Any individual run showed irregularities which by the exercise of imagination could be correlated with the known transitions, but the pressures at which the irregularities occurred showed no consistent pattern, and even the sign of the irregularities was not always consistent. The final results, which are given in Table XIII and Figure 11, have therefore been obtained by smoothing over any possible transition effects.

The failure of the known high pressure transitions to manifest themselves by changes of resistance is doubtless connected with the smallness of the volume changes. For convenience of reference the pressures and volume changes of the several transitions at 25° are listed.

Transition	Pressure kg/cm^2	$\Delta V/V_0$
I–II	25,650	0.0460
II–III	27,080	0.0295
III–IV	45,000	0.006
IV–V	65,000	0.005
V–VI	90,000	0.012

The volume changes at the high pressure transitions are thus small compared with the two at lower pressures, but if the discontinuity of resistance is proportional to the volume change they should nevertheless have been far beyond the errors of measurement. It would appear that the high pressure transitions are accompanied by a much less drastic change in the mechanism of conduction than the low pressure transitions.

Chromium. This was electrolytic material prepared at the Bureau of Standards, the identical material that was used in recently published[58] measurements of resistance to 30,000. For the present measurements pieces were ground to dimensions from the electrolytically deposited tube. There were brittle places in the tube as deposited. These were eliminated as far as possible by first cutting long strips from the tube and breaking these in the fingers into as short pieces as would break with the exercise of considerable force. The dimensions of the final specimens varied in thickness from 0.0027 to 0.0035 inch, and in width from 0.030 to 0.037 inch.

Four runs were made with two grades of carboloy. The changes of resistance at 100,000, in terms of the resistance at 30,000 as fiducial, varied from 0.128 to 0.141. In spite of this fair agreement, there was much more doubt than usual as to the best way of reducing these results so as to agree with the former ones to 30,000. Some of this doubt may be intrinsic in the material. Previous measurements had shown a sharp cusp at about 4,000 kg/cm^2 in the resistance-pressure relation, and furthermore the resistance at this cusp did not agree with previous low pressure anomalies exhibited by another specimen of presumably high purity in measurements to 12,000. Two methods of reduction were employed. In the one, the ratio of former resistance changes to the present were plotted as a function of pressure. This ratio increases roughly linearly with pressure to 50,000 (by extrapolation of former results). The results were corrected by using a factor running linearly from 1.00 at atmospheric pressure to 2.40 at 100,000. The second method of correction was to multiply the present changes of resistance by that constant factor which would reduce the present results to agreement with the former results at 10,000 and 30,000. These two methods of correction usually give closely concordant results, but here the first method gave for the resistance at 100,000 in terms of resistance at atmospheric pressure as unity the value 0.493, whereas the second method gave 0.623. In the absence of other compelling considerations the two methods were averaged to give the final values in Table XIV. The uncertainty in the final values ultimately stems from irregularity in the low pressure behavior.

TABLE XIV

Pressure kg/cm^2	R/R_0			
	Chromium	Molybdenum	Tungsten	Uranium
0	1.000	1.000	1.000	1.000
10,000	.827[a]	.987	.987	.947
20,000	.774	.975	.975	.904
30,000	.731	.963	.963	.866
40,000	.701	.951	.951	.837
50,000	.673	.940	.940	.812
60,000	.647	.929	.930	.790
70,000	.624	.919	.920	.770
80,000	.601	.909	.911	.752
90,000	.580	.900	.903	.737
100,000	.558	.892	.895	.724

[a] Cusp at 4,000; see text.

Molybdenum. The material was sheet 0.0015 inch thick from the stock of the Raytheon Manufacturing Company. It was annealed in vacuum in quartz at a white heat. Four runs were made with two grades of carboloy with two widths, 0.010 and 0.020 inch. The proportional changes of resistance at 100,000 in terms of the resistance at 30,000 as unity, varied from 0.076 to 0.098. The average was used. Compared with previous measurements[59] to 30,000 the present decrements of proportional resistance are too large. Agreement becomes better toward the higher pressures.

The ratio varies approximately linearly with pressure. The final results were obtained by using a correction factor increasing linearly with pressure from 0.54 at atmospheric pressure to 0.89 at 100,000. The results are given in Table XIV; there is no particular feature.

Tungsten. This material was 0.001 inch ribbon which I owe to the courtesy of Dr. C. G. Smith of the Raytheon Manufacturing Company. It was used without annealing and in a single width 0.035 inch. Four runs were made with two grades of carboloy. The decrement of proportional resistance at 100,000, in terms of resistance at 30,000 as unity, varied from 0.089 to 0.097. Compared with previous measurements[60] to 30,000 the present decrements are too large, the agreement becoming better at higher pressures. The ratio of the decrements increases linearly with pressure. A final correction factor was used increasing linearly from 0.42 at atmospheric pressure to 0.75 at 100,000. The final results are shown in Table XIV; they are without special feature.

Uranium. This was the same material as previously used in measurements of compression[61] to 100,000. To form the specimens a sliver was cut from the massive ¼ inch block with a knife, squeezed cold between carboloy platens to a thickness of 0.003 to 0.004 inch, and cut to widths of 0.03 to 0.04 inch. Four runs were made with two grades of carboloy. The high pressure results were fairly consistent. The proportional changes of resistance at 100,000, in terms of resistance at 30,000 as unity, were: 0.147, 0.171, 0.189, and 0.150. The low pressure results were not so consistent, however, the proportional change of resistance in the first 20,000 kg/cm^2 varying from 0.10 to 0.20. Some of this variation is perhaps to be associated with the non-cubic symmetry of the lattice.

There are no adequate previous results for comparison. The only previous measurements[62] are to 12,000 on a specimen of presumably much inferior purity. This specimen showed a linear drop of resistance with pressure with coefficient -4.36×10^{-6}, which at 20,000 would give a decrement of 0.087 against 0.10 to 0.20 above. I attempted without success to form wire suitable for new measurements to 30,000 by the same method of hot extrusion employed for neodymium and the other rare earths. The steel die reached its softening point before the uranium. Under the circumstances it seems that the best course is to give merely the average of the four runs without attempt at correction. The zero is extrapolated; the greatest doubt is at low pressure. These results are given in Table XIV. It will be seen that there is no feature in the results, and in particular no indication of any transition below 100,000.

Selenium. This was highly purified material which I owe to the kindness of Professor von Hippel of M.I.T. It was supplied to me in several forms of different heat treatments, but only one of these forms was used for the present measurements. This was in slender single crystal needles with a high metallic luster of the order of 2 cm long. An amorphous form prepared by heating to 200° C for two days had an initial resistance 10 times higher than the crystalline needles. Two runs were made on the crystal. For one of these a thin single crystal was squeezed between carboloy platens to about half thickness at 0.003 inch, and from this squeezed plate a roughly shaped specimen was picked out with a needle. For the other specimen a single crystal was fortunately found thin enough (0.0037 inch) to permit use without further thinning, and the specimen was trimmed out of this with a razor blade. The length of each specimen was along the original length of the crystal needle. The initial specific resistance at room temperature of the crushed needle was 4.2×10^3 and of the other specimen 3.64×10^3 ohm cm. The resistance of the specimens as mounted is initially very high, of the order of a megohm, so that a special arrangement of the circuits was necessary. Under 100,000 kg/cm^2 resistance decreases by something of the order of 10,000 fold, so that resistance has to be plotted on a logarithmic scale. The qualitative nature of the results was the same for both specimens. Plotted on a logarithmic scale, the curve drops with slight upward curvature to about 56,000 kg/cm^2. Here there is an obtuse upward knee, beyond which the curve drops linearly to 100,000. At the knee both runs showed a slight downward jump of resistance amounting to a drop of about 25 per cent. It would seem highly probable that the jump means a polymorphic transition, but if so a transition to another non-metallic form.

Previous measurements[63] on the volume to 100,000 on material originally in the glassy state had shown irregularities which were interpreted as due to some sort of permanent change. However, there is probably no correlation with the present results on crystalline material.

On a logarithmic basis the two runs were in fair quantitative agreement. for the one run $\log_{10} R_0/R_{100,000}$ having the value 3.95 and for the other 4.15. The two logarithms were averaged and the final results are shown in Table XV. There are no previous measurements to which these high pressure results can be corrected.

Tellurium. This material is of special interest because under normal conditions it is a semi-conductor which under pressure is pushed toward the metallic condition. Previous measurements[64] to 30,000 had given a decrease of resistance by a factor varying from 400 to 600 fold depending on the orientation of the single crystal. Furthermore, at 30,000 the temperature coefficient of resistance has become positive, like a metal.

Six different runs were made to 100,000 on three grades of material. Tellurium is excessively brittle. Five of the six runs were handled by the extruded wire technique, laying five or six lengths of wire 0.005 to 0.006 inch in diameter side by side in a trough pressed in the AgCl sandwich. The sixth run was on a thin sliver cleaved out of a single crystal, with length along the trigonal axis. The material of the sliver was old Kahlbaum stock. Three of the runs were made on the same old extruded stock on which previous measurements were made. This was made from as pure initial material as was then available and was purified further by crystallization, but I have no explicit data as to purity. Measurements on these four specimens differed greatly from the former measurements to 30,000, the drop of resistance between atmospheric pressure and 30,000 now being by a factor of only 5 against the 400 to 600 formerly. Accordingly the measurements were repeated with some highly purified tellurium made by distillation in helium which I owe to the kindness of Professor Harry Hall, by whom it had been prepared at Durham, N. H. Professor Hall found that $\log_{10}$ of the resistance of this material is linear in $1/T$ in the range —40° to 350°C, and the specific resistance satisfies the formula: $\rho = A \exp E_g/2\kappa T$, where $A = 8.90 \times 10^{-4}$ ohm cm, and $E_g = 0.323$ ev. It was formed into wire of 0.005 inch diameter by hot extrusion and two runs made with it. Resistance was now found to drop by a larger factor in the first 30,000 kg/cm² than for the other specimens, namely by factors of 16.1 and 19.3, but still is far short of the previous factors 400 and 600. So large a discrepancy cannot possibly be caused by any geometrical distortion associated with non-hydrostatic stress, and the only conclusion would seem to be that the effects of internal strain are unusually large for this material. In

TABLE XV

RESISTANCE OF SELENIUM

Pressure kg/cm²	$\log_{10} R/R_0$
0	0.00
20,000	− 1.26
40,000	− 2.22
56,000	− 2.85 − 2.95[a]
60,000	− 3.05
80,000	− 3.55
100,000	− 4.05

[a] Transition.

view of this it hardly seems worth while to reproduce the numerical results in any detail, and certainly not worth while to attempt to devise a method of correction that will secure agreement with the previous results.

Tellurium is known to have a polymorphic transition[65] with a 5.5 per cent volume discontinuity at 45,000. It is a natural surmise that the high pressure modification will be a metallic conductor. All of the present runs gave a large drop of resistance at a pressure never lower than 45,000 and never higher than 59,000. The lower pressures are more significant here because of the possibility of superpressing, and these results may be regarded as giving essential confirmation of the correctness of the present pressure scale. The factor by which the resistance dropped at the transition varied from 13.8 for the single crystal sliver to 26.7 for one of the highly purified specimens, average 20.1. So large a drop substantially confirms the hypothesis that the high pressure modification is metallic, as does also the absolute value of the specific resistance, which is approximately 13×10^{-6} ohm cm immediately above the transition. Above the transition all runs agreed in a smooth drop of resistance to 100,000. Logarithm of resistance is linear in pressure in this range. The variation between the different runs in this range was 6 per cent on both sides of the mean. In the range above the transition the mean rate of resistance drop is at the rate of 8.3 per cent for 10,000 kg/cm^2 pressure increase.

Manganese. Pure manganese has not, as far as I know, been produced in wire form. The particular difficulty is the polymorphic transition with volume change so large that the massive metal spontaneously fractures on cooling. Recently the Bureau of Mines investigated the possibility of various industrial uses for high grade manganese, and produced manganese of approximately 97 per cent purity in rod and wire form. I obtained some of this through the courtesy of Dr. Bruce Rogers. The composition was stated to be: Ni 1.02, Cu 2.33, S 0.1, balance thought to be practically all manganese, but not analyzed further. Because of the lesser interest in this material because of its inferior purity only two runs were made to 100,000 with two grades of carboloy. The specimens were prepared by grinding to a thickness of 0.0025 inch and width 0.037 inch from a wire 0.0625 inch in diameter.

The change of proportional resistance at 100,000 in terms of the resistance at 30,000 as fiducial, was 0.145 and 0.142. Both runs agreed in showing unusual curvature. Up to about 50,000 resistance decreases with normal upward curvature, but here there is a point of inflection followed by downward curvature with probably a second reversal and normal curvature again above 95,000.

The measurements to 30,000 went smoothly, with no perceptible hysteresis. The results, in terms of the usual $-\frac{1}{p}\frac{\Delta R}{R_0}$, are shown in Table XVI. The average temperature coefficient of resistance at atmospheric pressure between 0° and 25° was 0.00325. This is appreciably lower than the expected 1/T (0.00366); the difference is to be ascribed to the impurity. The specific resistance at atmospheric pressure and 0° was 6.36×10^{-5}.

The measurements to 100,000, reckoned from the resistance at 20,000 as fiducial, gave a smaller change of resistance below 20,000 and a larger change above than the 30,000 measurements, the ratio varying smoothly from 1.114 at atmospheric pressure to 0.932 at 30,000. This correction factor was extrapolated by smooth graphical construction to the value 0.73 at 100,000, and the results so corrected are given in Table XVII. There is obviously room for considerable doubt in the absolute values at the top pressure. Qualitatively, the results are somewhat unusual in the double reversal of curvature already mentioned.

TABLE XVI

RESISTANCE OF MANGANESE TO 30,000 KG/CM²

Pressure kg/cm²	$-\frac{1}{p}\frac{\Delta R}{R_0}$
0	4.22×10^{-6}
5,000	3.85
10,000	3.54
15,000	3.27
20,000	3.04
25,000	2.83
30,000	2.63

Iron. The material was "decarburized steel" which I owe to the courtesy of Dr. R. F. Mehl of the Carnegie Institute of Technology. It had been prepared by treating in wet hydrogen at 1200°C for one month. The original steel had the analysis: C 0.067, Mn 0.38, P. 0.014, S 0.024, Si 0.12. After treatment carbon was undetectable microscopically (less than 0.005 per cent), sulfur was probably less than 0.005 per cent, and hydrogen less than 0.0005 per cent. It was prepared for the present measurements by rolling from 0.04 inch to 0.002 or 0.001 inch thickness, with an annealing after rolling to red heat in quartz in vacuum, and was used in two widths, 0.020 and 0.010 inch. Six runs were made, of which two had to be discarded because of cracking of the carboloy platens. The four good runs were all made with the same grade of carboloy, #883.

The agreement of the four runs was good; the change of proportional resistance at 100,000, in terms of resistance at 30,000 as fiducial, varied from 0.100 to 0.105. Compared with previous measurements[66] to 30,000 the present decrements of resistance were too large, the reduction factor varying linearly with pressure from 0.80 at atmospheric pressure to 0.90 at 50,000 (extrapolated). The final results were obtained by using a correction factor varying linearly to unity at 100,000. The final results are given in Table XVIII. The proportional resistance at 100,000, in terms of resistance at atmospheric pressure as unity, given in the table is 0.841.

TABLE XVII

RESISTANCE OF MANGANESE TO 100,000 KG/CM^2

Pressure kg/cm^2	R/R_0
0	1.000
10,000	.965
20,000	.939
30,000	.921
40,000	.909
50,000	.900
60,000	.892
70,000	.883
80,000	.872
90,000	.856
100,000	.841

This is to be compared with the value 0.828 which would have been obtained if no correction procedure had been applied. The behavior of resistance is normal in every way, smooth drop with upward curvature.

Nickel. The material was cut from a single crystal which I owe to the courtesy of Dr. R. M. Bozorth of the Bell Telephone Laboratories. It is the same material as was used in recent measurements of compressibility[67] to 30,000. The purity is thought to be high, with no element present to as much as 0.01 per cent. A piece from the single crystal was rolled to 0.002 inch thickness, annealed to red heat in vacuum, and cut to widths of 0.010 and 0.020 inch for the measurements. Four runs were made with two grades of carboloy. The proportional change of resistance at 100,000, in terms of resistance at 30,000 as unity, varied from 0.099 to 0.111. The average was used. Compared with previous measurements[68] to 30,000 the present decrements of resistance are too large. The ratio increases roughly linearly with pressure; for the final results a correction factor was used increasing linearly from 0.78 at atmospheric pressure to 0.91 at

100,000. Former measurements showed a kink in the resistance curve at 10,000. This is on too small a scale to attempt to reproduce here, and the final results in Table XVIII are smoothed right through this point. Except for this small kink at 10,000 the resistance behaves conventionally.

Cobalt. This, like the nickel, was single crystal stock from Dr. Bozorth of the Bell Telephone Laboratories. It was "Katanga" cobalt, and a typical analysis showed 0.05 per cent iron and 0.01 per cent copper as the principal impurities. The specimens for the present measurements were prepared by cutting a thin disc from across the end of the single crystal, that is, with the principal axis perpendicular to the plane of the disc, swaging the disc down to a thickness of 0.006 inch by hot hammering, filing further to a thickness of 0.002 inch, annealing to red heat in vacuum, and cutting to widths of 0.022 and 0.047 inch for the measurements. Four runs were made with two grades of carboloy. The proportional change of resistance at 100,000, in terms of resistance at 30,000 as unity, varied from 0.041 to 0.047. The effect is therefore unusually small. The average of the four runs was used. Compared with previous measurements[69] to 30,000 the decrements of resistance were too large by a factor of nearly 2. This is not surprising in view of the absolute smallness of the effect. The correction factor showed a slight trend with pressure, and the final results, which are shown in Table XVIII were obtained with a correction factor increasing linearly with pressure from 0.55 at atmospheric pressure to 0.58 at 100,000. Cobalt was previously found to have certain irregularities at the low pressures, but these are on too small a scale to attempt to reproduce here. In addition to the smallness of the effect, cobalt is unusual in the flattening out of the curve, suggesting a minimum not far beyond

TABLE XVIII

Pressure kg/cm²	R/R_0		
	Iron	Nickel	Cobalt
0	1.000	1.000	1.000
10,000	.977	.982	.991
20,000	.956	.965	.983
30,000	.936	.948	.976
40,000	.919	.933	.970
50,000	.903	.918	.965
60,000	.888	.904	.961
70,000	.875	.892	.958
80,000	.862	.880	.955
90,000	.851	.869	.953
100,000	.841	.858	.951

100,000. Any conclusion with regard to such a minimum must, however, be used with considerable caution.

Rhodium. This material was originally obtained in 1923 from Johnson and Mathey, London, in the form of a bar swaged to dimensions at red heat. For the present measurements a piece was cut from the former compressibility specimen[70] and further hammered at a red heat to a thickness of 0.002 to 0.003 inch. Specimens were cut to widths of 0.030 to 0.040 inch, and four runs were made with two grades of carboloy. The proportional change of resistance at 100,000, in terms of resistance at 30,000 as unity, varied from 0.082 to 0.091; the average was used. Compared with previous measurements[71] to 30,000 the decrements are too large, the ratio showing a non-linear increase with pressure. The final results are given in Table XIX; they were obtained by using a correction factor varying from 0.656 at atmospheric pressure, to 0.888 at 50,000 to 1.000 at 100,000. Resistance drops smoothly with increasing pressure, and the behavior is entirely normal.

TABLE XIX

Pressure kg/cm^2	R/R_0			
	Rhodium	Palladium	Iridium	Platinum
0	1.000	1.000	1.000	1.000
10,000	.984	.980	.986	.981
20,000	.968	.960	.973	.963
30,000	.953	.942	.960	.947
40,000	.939	.925	.948	.931
50,000	.925	.909	.936	.916
60,000	.912	.894	.924	.903
70,000	.900	.881	.914	.891
80,000	.889	.869	.904	.880
90,000	.880	.858	.895	.870
100,000	.872	.847	.886	.861

Palladium. This material was originally obtained from Baker and Company in the form of wire 0.007 inch in diameter, and had been formerly[72] used in measurements of the effect of pressure on resistance at low temperatures. For the present measurements the wire was rolled flat to a thickness of 0.002 inch and degassed by heating to 900°C in vacuum. Four runs were made with two grades of carboloy. The proportional change of resistance at 100.000, in terms of the resistance at 30,000 as unity, varied from 0.098 to 0.102; the average was used. Compared with previous measurements of resistance[73] to 30,000 the decrements of resistance are too large, the agreement becoming better at higher pressures. The final results, shown in Table XIX, were obtained with a correction factor varying linearly with pressure from 0.81 at atmospheric pressure to 1.00 at 100,000. Resistance decreases normally with pressure.

Iridium. This was "pure" material from Baker and Company in the form of sheet 0.0016 to 0.0019 inch thick. Four runs were made with two grades of carboloy, the specimens varying in width from 0.020 to 0.037 inch. The proportional change of resistance to 100,000, in terms of resistance at 30,000 as unity, varied from 0.073 to 0.084. Compared with previous measurements[74] to 30,000 the decrements of resistance are too large, the agreement becoming better at the higher pressures. The final results were obtained by using a correction factor varying linearly with pressure from 0.586 at atmospheric pressure to 0.88 at 50,000, above which the factor was carried by smooth graphical extrapolation to 1.000 at 100,000. The results are shown in Table XIX; resistance decreases smoothly and normally.

TABLE XX

RESISTANCE OF Ag_5Cd_8 TO 30,000

Pressure kg/cm²	R/R₀
0	1.0000
5,000	.9858
10,000	.9737
15,000	.9630
20,000	.9532
25,000	.9440
30,000	.9354

Average temp. coef. 0°–25°C, .00237
Specific resis. at 0° at 0 kg/cm², 1.22 × 10^{-5} ohm cm

Platinum. The material was "iridium free" sheet stock 0.0015 inch thick from Baker and Company. After cutting to dimensions the specimens were annealed at white heat. Four runs were made with two grades of carboloy on widths varying from 0.010 to 0.020 inch. The proportional change of resistance at 100,000, in terms of resistance at 30,000 as unity, varied from 0.099 to 0.112; the average was used. Compared with previous measurements[74] to 30,000, the decrements of resistance are too large, the agreement becoming somewhat better at the higher pressures. The final results were obtained with a correction factor increasing linearly with pressure from 0.77 at atmospheric pressure to 0.86 at 100,000. The extrapolation of this factor beyond 30,000 cannot be regarded as very secure, but the details are of no great importance because of the narrow range of variation and the closeness of the factor to unity. Final results are shown in Table XIX; resistance decreases with pressure with normal upward curvature.

Intermetallic Compounds. Measurements were made on nine intermetallic compounds of comparatively simple structure. The resistance of seven of these has been previously measured[75] to 30,000, and that of one of the others, Ag_5Cd_8, is here measured to 30,000 and published in Table XX for the first time. The ninth, Cu_5Cd_8, has previously been measured[76] only to 12,000. Measurements on these nine compounds were much less complete than on the pure metals. Only one set-up was used with each, and the precise results are accordingly in more doubt. The measurements were undertaken primarily in the spirit of finding whether there were any new transitions of the first or second kind or other unusual features at high pressures, as perhaps might have been anticipated from the nature of the material. The results obtained should be competent to give information on these points. No such features were in fact found. The only noteworthy qualitative feature was a minimum in the resistance of Ag_5Zn_8; the minimum is so flat and the absolute magnitude of the pressure effect so small that the quantitative relations at this minimum as tabulated must be taken with great caution. The present uncorrected measurements to 100,000 gave a minimum at 70,000, whereas the former measurements to 30,000 located a minimum at about 27,000. The correction factor was determined so as to preserve the latter location of the minimum.

TABLE XXI

Pressure	R/R_0								
kg/cm²	CuZn	AgZn	AuZn	Cu_5Zn_8	Ag_5Zn_8	Cu_5Cd_8	Ag_5Cd_8	Ag_2Al	Sb_2Tl_7
0	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
10,000	.980	.963	.967	.969[a]	.996	.908	.974	1.001	.917
20,000	.961	.932	.938	.951	.994	.836	.953	1.003	.852
30,000	.944	.906	.912	.936	.994	.777	.935	1.005	.798
40,000	.928	.883	.889	.922	.995	.729	.921	1.008	.752
50,000	.915	.861	.869	.911	.999	.694	.910	1.011	.712
60,000	.903	.840	.851	.902	1.004	.664	.901	1.015	.676
70,000	.892	.819	.835	.894	1.012	.639	.894	1.019	.645
80,000	.883	.797	.822	.888	1.022	.619	.889	1.023	.616
90,000	.875	.775	.809	.883	1.034	.600	.885	1.029	.591
100,000	.868	.755	.799	.879	1.050	.585	.884	1.035	.568

[a] Cusp at 5,000.

In view of the less precise nature of the results on these intermetallic compounds, only the final results are presented in Table XXI without giving the details of the correction procedures for the different compounds, some of which must be recognized to have considerable uncertainty.

Iron-Nickel Alloys. Since I had on hand a fairly extensive material, seven compositions in all, an investigation was made in the same spirit as that on the intermetallic compounds, to discover whether there are any new qualitative features at high pressures. Five of the compositions were in the form of wire which I owe to the kindness of Professor McKeehan of Yale, and for which I have published measurements[77] of the effect of pressure on resistance up to 12,000. The wire was 0.010 inch in diameter. It was prepared for these measurements by annealing to a red heat, squeezing flat to a thickness of approximately 0.002 inch between carboloy platens, annealing again to a red heat and trimming to a width of 0.020 inch. The other two compositions were: (1) "permalloy," Fe 30, Ni 70, which I owe to the courtesy of Dr. Bozorth, and (2) "invar," of nominal composition Fe 62.5, Ni 37.5, from the stock of the laboratory machine shop.

TABLE XXII

RELATIVE RESISTANCE OF IRON-NICKEL ALLOYS

Pressure kg/cm²	Fe 90, Ni 10	Fe 80, Ni 20	Fe 75, Ni 25	Fe 70, Ni 30	Fe 62.5, Ni 37.5 Invar	Fe 60, Ni 40	Fe 30, Ni 70 Permalloy
0	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
10,000	.9997	.9609	1.0273	1.0866	1.0243	1.0849	1.0045
20,000	.9945	.9547	1.0640	1.1998	1.0327	1.1592	1.0082
30,000	.9858	.9550	1.0987	1.3166	1.0269	1.2154	1.0107
40,000	.9781	.9566	1.1272	1.4381	1.0128	1.2540	1.0132
50,000	.9716	.9587	1.1513	1.5160	.9953	1.2771	1.0169
60,000	.9661	.9598	1.1746	1.5986	.9784	1.2854	1.0205
70,000	.9614	.9618	1.1934	1.6690	.9640	1.2858	1.0265
80,000	.9575	.9642	1.2116	1.7310	.9523	1.2819	1.0330
90,000	.9540	.9664	1.2287	1.7820	.9480	1.2747	1.0408
100,000	.9510	.9680	1.2514	1.8262	.9293	1.2650	1.0482

Only one run was made on each composition. In the five cases where previous measurements had been made to 12,000 the present results were corrected to give agreement over the common range. The correction was usually made in the simplest possible way with a factor constant over the entire range to 100,000. There are no previous measurements on permalloy or invar, and the present results are here given without correction.

The results are listed in Table XXII. Figure 13 gives a general survey of the results. In the top part of the figure the results for the different alloys are shown qualitatively, *not* to scale, and with exaggeration of the curvatures; in the bottom part of the figure the results are all given together on the same scale. It will be seen that unlike the intermetallic compounds there is here a wealth of new feature at high pressure, commonly taking the form of unusual reversals of curvature. It suggests itself that alloys offer a fertile field for future study.

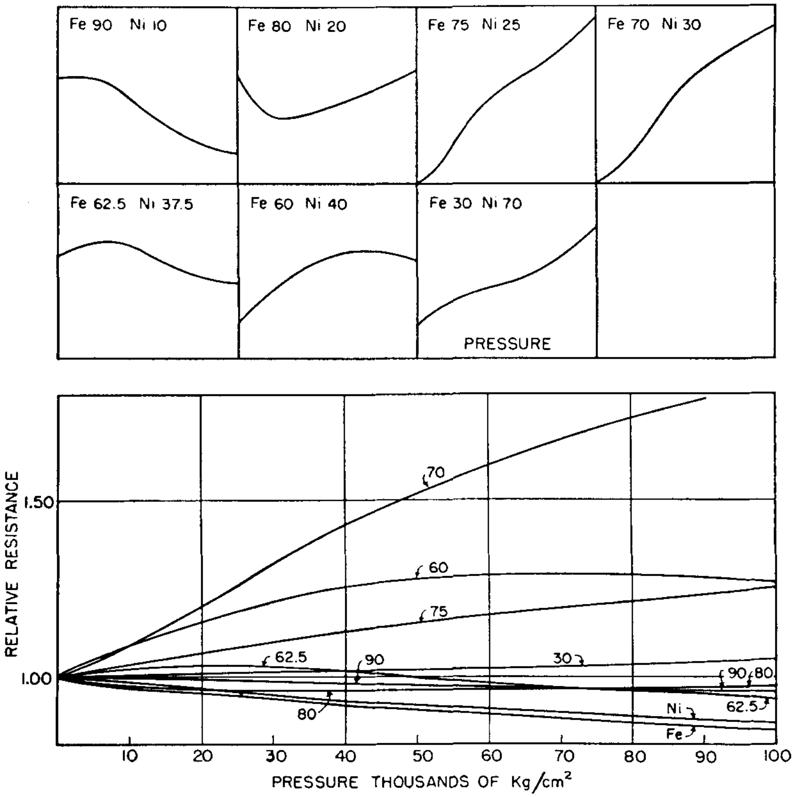


FIGURE 13. The resistance to 100,000 kg/cm² of a series of iron-nickel alloys. In the upper part of the diagram the curves are exaggerated so as to bring out the qualitative features; in the bottom part of the diagram the relative resistances of the various alloys are shown to scale.

In my notes I found a determination of the compressibility of invar at 30° to 12,000 which apparently has not yet been published. I take the occasion to give it here. Using all corrections for the most recent values for iron the formula is:

$$-\Delta V/V_0 = 9.96 \times 10^{-7}p - 4.62 \times 10^{-12}p^2, \qquad p \text{ in kg/cm}^2.$$

There follow now three semi-conductors. The specific resistance of these is so low that no error is introduced by the conductivity of the AgCl.

Silver Sulfide. The resistance of Ag_2S under pressure has already been measured [78] by Montén to 3,000 kg/cm², who found that the resistance drops at 3,000 by a factor of approximately 15, with much curvature in the pressure-resistance relation.

Material for the present measurements was prepared by heating Kahlbaum's "K" sheet silver, 0.010 inch thick, to 300° for 2 hours submerged under Kahlbaum's "K" sulfur. The metal appeared completely transformed. A piece 0.004 inch thick was filed to a width of 0.030 inch and mounted in the regular way in the AgCl sandwich. The initial resistance was of the order of megohms, so that a special arrangement of the circuits was necessary. One successful run was made. The resistance at first drops very rapidly with pressure. Up to 25,000 the relation between pressure and logarithm of resistance is linear, resistance dropping by a factor of 6,500 at 25,000 kg/cm². This would mean a drop by a factor of 2.9 for 3,000, much less than the factor 15 found by Montén. At 25,000 the curve of log(resis) against pressure becomes concave upward and continues smoothly to 58,000, where resistance has dropped by a factor of 180,000 compared with initial. At 58,000 an accelerated drop begins, continuing to 67,000, where resistance has dropped by an additional factor of 10. The presumption is that this accelerated drop means a polymorphic transition. At 67,000 the accelerated drop ceases and from here to 100,000 resistance drops by a further 35 per cent with normal upward curvature. The behavior above 67,000 is thus qualitatively like that of a metal, and the presumption is that the high pressure modification is metallic. On release of pressure the transition and the very large change of resistance at low pressure were confirmed.

Copper Sulfide (*Cu_2S*). Only one run was made. The specimen was ground out of a slug which I had synthesized many years ago by heating together weighed quantities of the elements. The thickness was 0.0035 inch and the width 0.065. Compared with silver sulfide the initial resistance is much lower and the effect of pressure much less. Up to about 32,000 the logarithm of resistance decreases approximately linearly with pressure, the total drop of resistance in this pressure range being by a factor of 8. At 32,000 an accelerated *rise* of resistance begins, continuing to 40,800 with total increase of resistance by a factor of 9, that is, bringing the resistance back to 12.5 per cent above the initial value. From 40,800 to 85,000 logarithm of resistance drops with minor irregularities and strong upward curvature on the average by a factor of 2.5 on the resistance. Between 85,000 and 100,000 there is an accelerated drop by an additional factor of 2.7 on the resistance, probably due to another transition. On release, the presence of the two transitions was confirmed, but not the details of the variations.

Molybdenite (MoS_2). The material was natural crystals originally obtained by Professor G. W. Pierce many years ago from Eimer and Amend in connection with his experiments on rectifying contacts. For the present measurements it was cleaved to 0.0037 inch thickness and used in the form of a button 0.047 inch in diameter as the middle of a AgCl sandwich with three layers. The initial specific resistance was approximately 180 ohm cm. In view of the nature of the material only a single run was made. Resistance decreases with normal upward curvature from atmospheric pressure to approximately 20,000, where the resistance is 0.557 initial. Here there is an abrupt drop, doubtless due to a transition, by a factor of 2.77, to a resistance 0.201 of initial. From here to 100,000 resistance drops smoothly with normal upward curvature to a value 0.110 initial. The transition was confirmed on release of pressure. There are no previous measurements on this material for comparison.

SUMMARY AND CONCLUSION

The measurements just presented include 52 elements, of which 47 may be regarded as metallic. The five non-metallic elements are: carbon, silicon, germanium, selenium, and tellurium. Of the 47 metallic elements, 31 exhibit "normal" behavior of resistance under pressure, that is, a resistance decreasing with pressure with upward curvature. Included in these 31 normal metals are four with phase changes; mercury, with one freezing and one polymorphic transition; gallium, with one freezing and one transition, thallium, with one transition, and zirconium, with one transition. The discontinuity of resistance at the phase changes of these four metals also behaves "normally," that is, resistance follows the volume, decreasing with it, at the phase change. The high pressure phases of these four elements also show normal curvature (presumptive for the high pressure form of zirconium). For two of the 31 "normal" metals the decrease of resistance with pressure is not smooth over the entire range, chromium and nickel both having obtuse cusps at low pressures. The magnitude of the change of resistance of the 31 normal metals under a pressure of 100,000 kg/cm^2 varies over a wide range, being approximately 50 per cent for tin and lead and 5 per cent for cobalt and neodymium. The over-all changes of resistance of thallium and mercury are larger than 50 per cent, but these are not comparable because of the phase change. In particular, at the freezing of mercury resistance drops by a factor of 4. In general the change of resistance is less for the high melting hard metals. Certain sets of metals are very similar: tungsten and molybdenum are indistinguishable; tantalum and columbium are very close to each other, as are also the four noble metals rhodium, palladium, iridium, and platinum.

The 16 "abnormal" metals show a wide variety of behavior. In the first place, at the phase changes of these metals resistance does not universally follow the volume, but there are three examples where resistance increases with a decreasing volume: caesium (at 22,000 kg/cm^2), barium, and bismuth (II–III).

The simplest "abnormality" is a resistance increasing with pressure. Formerly I had made the generalization that all resistance curves, whether rising or falling with pressure, could be regarded as pieces of the same curve, namely a curve concave upward. All known rising curves, with the exception of a particular orientation in single crystal antimony, were of this type, as were also all known falling curves. If this type of behavior were indeed general, then it was to be expected that presently the falling curves would pass through a minimum, and that eventually resistance would rise with pressure for all metals. This expectation was strengthened by the known minima for potassium, rubidium, caesium and barium. These new measurements over a three fold more extensive pressure range negative this generalization and add but another to my failures to prophesy new pressure effects when extending the range. Calcium is now the only metal with a smoothly increasing resistance with upward curvature over the entire pressure range. Barium would be included here except for the polymorphic transition. The resistance of high pressure barium also increases with upward curvature, but the rate of increase is very much less than that of the low pressure form and the curvature is very slight. It is to be remembered that the direction of change of resistance at the transition of barium is abnormal. Of the other metals with ultimately increasing resistance, the resistance of potassium increases smoothly over the entire range after the minimum with upward curvature, as does also that of sodium beyond the minimum, here found for the first time in about the expected place. Lithium and rubidium, however, do not rise with smooth upward curvature, but both have an intermediate episode of reversed curvature with two points of inflection. Below the electronic transition of caesium the curvature of both modifications is upward. It was unexpected to find a cusp at the electronic transition instead of a discontinuity, since the measurements of volume had given a sharp and large volume discontinuity. Instead, the effect of the electronic transition on resistance is smeared through a pressure range of 50,000 kg/cm^2. It is to be considered whether the inflections and reversals of curvature of lithium and rubidium are to be explained by some sort of electronic rearrangement, analogous to but less drastic than that of caesium. The maximum of strontium involves a reversal of curvature followed by a second reversal with ultimate rise of resistance with upward curvature. Comment has already been made on the fact that the pattern of resistance change is quite different from that of volume change under pressure, the latter showing two discontinuities at pressures straddling the maximum of resistance. In explanation the possibility of abnormally large effects from the shearing components of stress is to be considered.

The four rare earth metals, lanthanum, cerium, praseodymium and neodymium are all abnormal and the abnormality of each is of a different type. Initially the resistance of lanthanum drops with downward curvature (one of very few examples), then passes through a much smeared out transition, and eventually passes through a minimum and ultimately rises with upward curvature. Cerium presumably passes through an "electronic" transition with large drop of resistance beyond which there is a minimum with rise terminated by a cusp beyond which resistance drops. The resistance of praseodymium at first passes through a very flat minimum, followed by a cusp and a second minimum. The former volume measurements had shown no volume discontinuity corresponding to the cusp. The resistance of neodymium varies over the entire range less than any of the other metals studied here except cobalt, and passes through a very flat minimum at about the middle of the range.

The three metals arsenic, antimony and bismuth all show abnormal behavior. Arsenic in most orientations passes through a minimum followed by a maximum, the curvature still being downward at the top pressure; this curvature must of course reverse at higher pressures. There is one orientation of arsenic in which the resistance drops over the entire range, but with reversals of curvature paralleling those of the mean orientations with minimum. The curvature of this one orientation also remains abnormally downward at the top pressure. The resistance of a presumably average orientation of antimony at first rises with the usual upward curvature, which presently reverses with a maximum and an eventual second reversal of curvature so that at the top pressure resistance is dropping with normal upward curvature. Bismuth at first increases with usual upward curvature; at the first transition there is an abnormally large drop, at the second transition an abnormal rise, followed at the third transition by a normal drop, beyond which resistance drops normally over the rest of the range with normal and slight upward curvature. No trace was found of the further two transitions in this range known from the volume measurements.

Finally, of the abnormal metals, the resistance of manganese at first drops with normal upward curvature followed by a reversal to downward curvature, followed again by a second reversal back to normal upward.

The full significance of this is not clear because of the two per cent admixture of copper, but it is to be remembered that the crystal structure of manganese is highly complex and abnormality is therefore not surprising.

Consider next the non-metallic elements. Carbon in the form of single crystal graphite drops in resistance at first with normal upward curvature, followed by reversal to abnormal downward curvature which persists to the highest pressure. The numerical magnitude of the change for graphite is that of a median metal. For the behavior of the semi-conductors silicon and germanium there seems no simple generalization; results on different specimens have not the desired reproducibility, the behavior alters entirely depending on whether the conductivity is n-type or p-type, and there are large variations of resistance with marked episodes for which the detailed description in the body of the text should be consulted. The resistance of selenium and tellurium drops so rapidly with pressure that a logarithmic scale has to be used. Further, both show abrupt drops at a median pressure indicating polymorphic transitions, that of tellurium coinciding with one already known. Beyond the transition of tellurium the behavior is like that of a normal metal. The absolute values of the changes of resistance for these two substances found under the present arrangements are much less than those previously found with pressure transmitted by a true liquid, so that it is probable that the effects of non-hydrostatic components of stress are unusually important here.

Turning now to the nine intermetallic compounds, these, contrary to expectation, showed no particular features. Two of these rise in resistance with pressure — the other seven fall. The curvature of all is normal — upward, except for a very slight reversal at the upper end of the range by AgZn. The curvatures do not vary regularly with the absolute magnitude of the effect, but there are several examples of crossing of the resistance curves. The range of numerical magnitudes is within that exhibited by the metallic elements.

The iron-nickel alloys somewhat unexpectedly showed more feature than the intermetallic compounds. These have been sufficiently summarized in the text; reversals of curvature abound.

The three semi-conductors Ag_2S, Cu_2S, and MoS_2 show no single type of behavior; the behavior of all is complicated by the entrance of new polymorphic forms. In general comment on the semi-conductors, the magnitude of the pressure effects is much larger than for the metals.

In general comment on the results, the wealth of feature at high pressures is striking. This wealth of feature does not include any special tendency for resistance to pass through a minimum with pressure, as had been anticipated from measurements below 30,000. However, a number of examples of minima were found. With regard to wealth of feature, it is obvious that the mechanism responsible for resistance is much more sensitive in this regard than the mechanism responsible for change of volume, previous measurements of volume to 100,000 kg/cm^2 having given no intimation of the feature shown by the resistance. It would seem that the time is now ripe for another theoretical attack on the general problem of the effect of pressure on resistance. It would seem highly probable that electronic rearrangements within the lattice or the atom will be important, something already suggested by caesium and cerium. It would seem natural to associate the various reversals of curvature of other metals with analogous electronic rearrangements, less drastic than for caesium and cerium. In this connection the abnormal results for the four rare earth metals is suggestive, the electronic structure of the atoms of these metals being such as to offer particularly fertile opportunity for rearrangement.

I am especially indebted to Mr. Charles Chase, without whose unusual skill in assembling and constructing the apparatus these measurements would not have been possible.

Lyman Laboratory of Physics,
Harvard University, Cambridge, Mass.

REFERENCES

1. P. W. Bridgman, Proc. Amer. Acad. 74, 425–440, 1942; 76, 1–7, 1945; 76, 55–87, 1948.
2. P. W. Bridgman, Proc. Amer. Acad. 71, 387–460, 1937.
3. Reference 2, p. 442.
4. P. W. Bridgman, Proc. Amer. Acad. 57, 41–66, 1922.
5. Second reference under 1, p. 4.
6. P. W. Bridgman, Proc. Amer. Acad. 72, 157–205, 1938; 79, 125–179, 1951.
7. First reference under 6, p. 176.
8. First reference under 6, p. 177.
9. First reference under 6, p. 177.
10. First reference under 6, p. 179.
11. Third reference under 1, p. 61.
12. First reference under 6, p. 183.
13. First reference under 6, p. 172.
14. First reference under 6, p. 172.
15. First reference under 6, p. 172.
16. P. W. Bridgman, Proc. Amer. Acad. 77, 208, 1949.
17. Second reference under 6, p. 152.
18. First reference under 6, p. 184.
19. First reference under 1, p. 436.
20. First reference under 6, p. 186.

21. First reference under 1, p. 436.
22. First reference under 6, p. 187.
23. First reference under 1, p. 436.
24. Second reference under 6, p. 153.
25. W. A. Zisman, Rev. Sci. Instr. 3, 367–370, 1932.
26. First reference under 6, p. 192.
27. P. W. Bridgman, Proc. Amer. Acad. 52, 591, 1917.
28. P. W. Bridgman, Proc. Amer. Acad. 60, 346, 1925.
29. P. W. Bridgman, Proc. Amer. Acad. 47, 347–438, 1912.
30. P. W. Bridgman, Phys. Rev. 48, 896, 1935.
31. First reference under 6, p. 189.
32. Second reference under 6, p. 153.
33. P. W. Bridgman, Proc. Amer. Acad. 56, 104–112, 1921; Phys. Rev. 48, 900, 1935.
34. Second reference under 6, p. 161.
35. Reference 30, p. 898.
36. Reference 27, p. 588.
37. P. W. Bridgman, Proc. Amer. Acad. 62, 207–226, 1927
38. Second reference under 6, p. 162.
39. P. W. Bridgman, Proc. Amer. Acad. 76, 75 and 65, 1948.
40. Second reference under 6, p. 164.
41. Second reference under 6, p. 168.
42. Reference 39.
43. Second reference under 6, p. 169.
44. Second reference under 6, p. 153.
45. P. W. Bridgman, Proc. Amer. Acad. 63, 347–350, 1928.
46. Reference 37, p. 218.
47. P. W. Bridgman, Proc. Amer. Acad. 56, 123, 1921.
48. Second reference under 6, p. 142.
49. Second reference under 6, p. 139.
50. First reference under 6, p. 193.
51. First reference under 6, p. 175.
52. Second reference under 6, p. 160.
53. Second reference under 6, p. 169.
54. P. W. Bridgman, Proc. Amer. Acad. 68, 47, 1933.
55. First reference under 6, p. 195.
56. First reference under 1, p. 438.
57. First reference under 6, p. 196.
58. Second reference under 6, p. 155.
59. Second reference under 6, p. 160.
60. Second reference under 6, p. 170.
61. Reference 39, p. 66.
62. P. W. Bridgman, Proc. Amer. Acad. 58, 158, 1923.
63. First reference under 1, p. 438.
64. First reference under 6, p. 200.
65. Reference 30, p. 900.

66. First reference under 6, p. 175.
67. P. W. Bridgman, Proc. Amer. Acad. 77, 203, 1949.
68. Second reference under 6, p. 156.
69. Second reference under 6, p. 159.
70. P. W. Bridgman, Proc. Amer. Acad. 59, 109–115, 1923.
71. Second reference under 6, p. 160.
72. P. W. Bridgman, Proc. Amer. Acad. 67, 331, 1933.
73. Second reference under 6, p. 161.
74. Second reference under 6, p. 170.
75. Second reference under 6, pp. 171–174.
76. P. W. Bridgman, Proc. Amer. Acad. 70, 291, 1935.
77. P. W. Bridgman, Proc. Amer. Acad. 63, 329–345, 1928.
78. F. Montén, Thesis, Uppsala, 1909, Almquist och Wiksells Boktryckeri.

81—5

Proceedings of the American Academy of Arts and Sciences

Vol. 81, No. 5, P. 252–282 — March, 1952

REPORT ON THE WHITE DWARF SURVEY OF THE SOUTHERN HEMISPHERE

By Willem J. Luyten

REPORT ON THE WHITE DWARF SURVEY OF THE SOUTHERN HEMISPHERE

By WILLEM J. LUYTEN

1. One of the primary purposes in undertaking the Bruce Proper Motion Survey of the Southern Hemisphere was to provide the basic material from which a further search for White Dwarfs could be started. Naturally the probability of finding white dwarfs is greatest among the stars of largest proper motion; all stars with motions in excess of 0″.5 annually were therefore observed first. Color observations on these stars were made possible through the cooperation of Dr. E. F. Carpenter at the Steward Observatory, Tucson, Arizona and Dr. E. Gaviola and Mr. Martin Dartayet of the Cordoba Observatory in Argentina. These observations were completed by 1945 and a total of 26 new white dwarfs were found.

Since then the color survey has been extended to include (a) all stars with motions between 0″.3 and 0″.5 annually, and (b) all components of wide pairs with common proper motion. All observations under (a) for stars south of declination −50 have been completed at Cordoba by Mr. David McLeish and 23 new white dwarfs have been found and announced. Observations for the stars with declinations north of −40 are in progress at Tucson but their completion will take many more years especially since to date motions have not been measured for stars north of declination −20. All stars under (b) within reach of the 36″ telescope at Tucson have been observed by Dr. P. D. Jose and the writer and 11 new white dwarfs have been found and announced.

2. When the completion of these several programs was approaching it became evident that the efficiency attainable by this type of observation — where only the field of one proper motion star at a time was photographed with a large telescope — was also reaching its limit. Obviously, color observations, one by one of, say, stars of the sixteenth magnitude with proper motions of 0″.050 annually do not constitute an efficient way of finding new white dwarfs, nor even an efficient way of utilizing a large telescope. Since a few probable white dwarfs had been found among faint stars with rather small proper motions and also because of the signal success attained by Humason and Zwicky in their search for blue stars in high galactic latitude a plan was conceived to rephotograph the entire southern hemisphere on yellow-sensitive emulsions down to the limit of the original proper motion survey. If such plates were taken with the same Bruce telescope with which the original proper motion plates had been taken and if the yellow plate was compared with one of the blue plates in the blink

microscope it was hoped that a color survey of all the stars found to possess appreciable motion could be completed in a reasonably short time.

3. To put this plan into execution two grants were received from the Rumford Fund of the American Academy of Arts and Sciences which were sufficient to provide all the yellow plates necessary for coverage of the region south of declination −45. The Harvard Observatory kindly offered to have these plates taken as part of its regular observing program and all 361 plates required were actually taken between 1947 and 1950 — before the Bruce telescope was dismantled.

All plates used were of emulsion 103aE and were taken through a #12 "minus blue" filter and given uniform exposures of 45 minutes. One of the pleasant surprises encountered was that these new yellow emulsions were so fast that, except for some unavoidable accidents due to sky-transparency, exposure, or development, the new 45 minute exposures show virtually all stars found on the old blue plates, some of which had exposures up to four hours. The old plates were all 14 × 17 inches in size, and were taken in such a way as to provide a considerable overlap in right ascension but only a very small one in declination. The largest yellow filter that could be obtained for this program measured only 11 × 14 inches in size; consequently a few stars were lost at the extreme north and south sides of the plates, but virtually none on the east or west sides.

The present set of yellow plates, in conjunction with the earlier blue plates constitutes, therefore, the first virtually complete color coverage down to the sixteenth magnitude for any large part of the sky.

4. All plates south of declination −60, numbering 169 in all have now been examined and the present paper gives a summary of the results obtained and the conclusions reached from that examination. For each of the 169 regions the oldest blue plate — generally taken around 1899 — was compared with the new yellow plate in the blink microscope. In the majority of cases the markings identifying the proper motion stars had been preserved on the glass side of the old plates but in any case the nearly 50 year interval between the blue-yellow pair served as an additional check. This not only greatly reduced the time spent on the otherwise tedious job of identification — especially so in rich Milky Way fields — but also ensured complete certainty of identification in doubtful cases as well as reascertaining the reality of the originally recorded proper motion — in direction as well as in approximate amount at the same time that the color comparison was made.

Since the majority of proper motion stars is red, their images are generally stronger on the yellow plates than on the blue plates whereas the reverse is true for stars whiter than the background. These latter therefore stand out in the usual blink procedure because their apparent variability goes contrary to the general run. In addition, the blink procedure followed undoubtedly allows the observer to see and estimate colors for stars very much fainter than would be the case if the two plates were compared side by side. In fact, this observer is convinced that he can thus estimate the color of many stars which are really invisible on the yellow plates — provided, of course, they are not too much invisible.

5. Where the number of stars to be examined is close to 40,000 it would obviously not have been feasible to make accurate estimates of the color index for each star. After a few trials it was found that a rough classification into four groups could be made quickly enough to enable one observer to complete the survey in a few years and yet obtain results which appear to be significant. These four groups are:

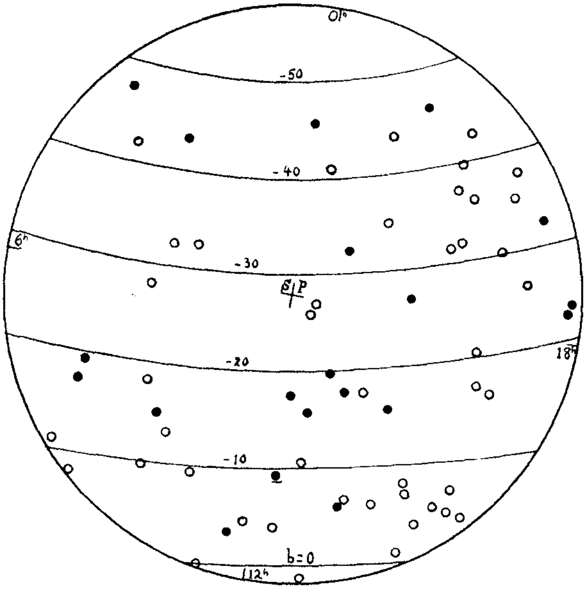


FIGURE I. Distribution of seventy probable White Dwarfs south of −60°.
Dots: stars with proper motions larger than 0″.3 annually.
Open Circles: stars with proper motions less than 0″.3 annually.

(a) Stars that stand out as extremely red; these probably comprise mostly stars with spectral class M and even late M. Nearly all the faint stars with very large proper motions belong to this group.

(b) Stars which appear somewhat redder than the background stars; probably comprising chiefly late K and early M spectra.

(c) Stars of about the same color as the background stars, and probably mostly belonging to spectral classes G and K.

(d) Stars definitely less red than the background. Among these the real white dwarfs are to be found.

By using again the original ledgers employed in blinking the plates for proper motion it was found that examination and recording of the stars in the above groupings could be accomplished in somewhat less than one minute per star. Groups a–c were recorded in these original ledgers in red pencil as R, r, nvr (not very red) respectively; these designations are now also being transferred to the permanent filing cards in the general catalogue. The stars of group d are the only ones meriting individual attention; for them a rough estimate of the color index relative to from five to ten comparison stars was made to the nearest $0^{m}.1$ and recorded in blue pencil. It cannot be emphasized too strongly that there remains an element of arbitrariness and subjectivity in this procedure: on some days the observer is much more "generous" in his classifying stars under group d than on others. It would seem to the present writer that, short of adopting an elaborate and time-consuming procedure involving accurate measurements of the color index for each star this subjectivity is merely one of the penalties one has to pay if the whole survey is to be completed in a reasonable time. Moreover it is doubtful whether any real accuracy could be obtained with these plates, taken as they are through a yellow filter, with a four component lens corrected for photographic light (the image of Alpha Centauri is fully one centimeter in diameter). The uncertainties introduced by the observer's erraticness — or by the variability of the plates or the instrument — may ultimately be estimated and corrected for by an analysis of the color estimates made for the same star on two different, overlapping plates, and there should be several thousand cases of this kind.

Some by-products of the present survey may be briefly mentioned:

(a) Many errors in the determination of position, magnitude, cataloguing and even of identification have come to light as a result of the color survey.

(b) In the original survey many stars were indicated as possibly possessing a proper motion but some doubt remained even after measurement as often the uncertainty was due to an irregularity of the image on one of the plates. Now that a fifty-year interval instead of a thirty-year one is available many of these doubtful motions can be checked — shown to be either definite or illusory. Also, as has happened in several instances, the finding of a faint star with a doubtful proper motion as very white is usually a good indication that the motion is spurious.

6. Perhaps the most outstanding impression obtained from the examination of the first 169 pairs of plates is the extraordinary redness of some Milky Way regions where almost every proper motion star is *less* red than the background. Often this redness due to obscuration can be recognized, more often perhaps it is not and then merely adds to the uncertainties in the resulting color indices which obviously must be regarded as highly provisional. Even so, it has been thought worth while to publish detailed results for all stars classified under group d. These have been divided into two further groups, the first, and much the smaller one comprising those stars which from an analysis of their magnitude, proper motion and color appear to possess the greatest probability of being genuine white dwarfs. Owing to the very considerable uncertainties in the adopted color indices no one, and certainly not the present writer would believe that they all actually are white dwarfs. Possibly no more than one third of them are genuine white dwarfs while the others more properly belong to that group of stars intermediate between the main sequence and the white dwarfs, some perhaps may be ordinary red main-sequence stars lying in heavily obscured regions where insufficient allowance has been made for the redness of the background. While the stars herewith announced as probable white dwarfs may thus be individually in error the present writer believes that, statistically, they represent the best estimate in number, and kind that can now be made from the existing material. For every star which ultimately will be rejected from this list another should be found from the much larger number in the general group d to take its place.

Incidentally, it may be pointed out that since the blue and yellow plates used were taken at widely different times, the possibility that an occasional eclipsing variable slips in as a white dwarf should not be overlooked.

Data for those stars, 44 in number, which had not previously been announced as probable white dwarfs are shown in Table I where the different columns give, in order, the star's designation, both the BPM and L numbers, the position for 1950, the apparent photographic magnitude, the total annual proper motion and its direction, and the estimated color index.

Table II shows, similarly, the data for the 27 white dwarfs south of declination −60 announced previously; the last column here gives the reference to the discovery announcement. All these stars were found again in the present survey with the exception of BPM 7756, 11534, and 13870. These stars are so faint — and so white — that they do not show on the present yellow plates; but in each case the notation made at the time of the color examination was: "invisible, probably blue". This suggests that a few white dwarfs may have been similarly missed because they are too

TABLE I

BPM	L	R.A. 1950	Dec	m	μ	p	I.C.
		h m	° ′		″	°	
3116	91–71	3 42.0	−67 19	15.3	0.111	30	0.0
3523	31–99	4 46.5	−78 57	13.1	.061	255	−0.1
3638	31–34	5 06.6	−76 38	16.0	.124	167	0.2
4225	32–11	6 24.4	−75 40	15.1	.086	17	0.1
4400	33–11	7 01.5	−75 13	14.3	.062	127	0.2
5102	63–60	8 48.1	−73 02	15.2	.111	291	0.0
5109	139–26	8 50.0	−61 43	14.5	.093	300	0.0
5401	140–38	9 17.6	−60 36	14.6	.046	8	0.0
6082	64–27	9 54.6	−71 02	14.4	.174	215	0.0
6114	101–26	9 57.8	−66 39	15.1	.072	344	0.0
6502	101–80	10 42.6	−69 02	14.0	.278	267	0.1
6961	67–5	11 26.8	−70 32	12.6	.114	254	0.0
7263	103–45	11 57.5	−66 04	16.0	.130	312	0.1
7543	104–2	12 23.8	−65 56	15.0	.186	182	0.0
7961	69–47	12 57.9	−72 18	15.0	.187	100	0.0
8474	148–42	13 40.7	−61 59	14.6	.067	115	0.1
9040	150–52	14 36.9	−62 45	16.2	.133	228	0.0
9085	107–29	14 41.6	−66 10	15.5	.10:	210	0.2
9164	108–66	14 49.3	−67 03	14.9	.058	297	0.0
9216	151–81	14 54.0	−63 05	17.0	.044	180	0.2
9306	151–60	15 02.1	−62 34	15.9	.069	277	0.2*
9323	41–26	15 04.9	−77 00	15.5	.071	54	0.1
9402	151–31	15 11.5	−61 42	15.5	.071	232	0.3
9483	108–124	15 20.0	−68 47	15.0	.110	110	0.1
9949	153–155	16 06.1	−63 14	13.2	.085	44	0.1*
10197	154–122	16 28.7	−62 21	16.2	.087	190	0.2
890	8–61	16 28.8	−87 17	15.0	.085	51	0.0
10552	111–44	17 02.4	−67 05	13.4	.061	173	0.2
10564	111–75	17 03.7	−68 41	14.9	.106	168	0.2
921	7–44	17 08.8	−87 08	15.0	.156	60	0.0
11316	157–64	18 15.3	−62 35	15.8	.062	194	0.2
11655	113–132	18 37.3	−69 31	15.6	.126	188	0.4
11995	114–29	18 57.8	−65 32	14.6	.086	177	0.2
12527	114–353	19 30.1	−67 16	14.8	.129	194	0.3
12772	79–51	19 48.9	−71 57	15.8	.140	168	0.3
12843	80–56	19 53.2	−71 31	15.4	.219	180	0.0
13163	162–217	20 19.1	−64 34	16.8	.126	184	0.3
13491	116–79	20 39.6	−68 16	13.5	.247	138	0.0
13537	162–160	20 43.0	−63 31	16.2	.044	131	0.1
13673	117–181	20 54.7	−69 09	16.9	.063	148	0.3
13853	47–70	21 10.9	−77 37	15.6	.042	144	0.2
14184	164–165	21 34.8	−64 45	15.0	.153	164	0.3
14942	83–2	22 35.0	−69 57	14.0	.068	186	0.2
15727	26–23	23 37.7	−76 03	14.8	.260	241	0.4

* Probably strong obscuration in these fields.

faint to show on the present yellow plates. For BPM 1266= L24–52 the Harvard color plates indicate a considerably yellower color than did the Cordoba plates.

The remaining stars of group d were all found to be less red than expected but not blue enough to consider them as probable white dwarfs. Stars of high luminosity and large, positive color-index are universally called red giants; in the accepted way of drawing the Hertzsprung-Russell diagram these stars lie at the upper right. Stars of very low luminosity and negative or only slightly positive color index are generally called white dwarfs; these occupy the lower left corner of the H-R diagram. But here the consensus of opinion ends and confusion begins. Most astronomers term the locus in the H-R diagram running from upper left to lower right, i.e., from the highly luminous O stars to the feebly luminous M stars the

TABLE II

BPM	L	R.A. 1950	Dec	m	μ	p	I.C.	H.A.C.
		h m	° ′		″	°	m	
1595	50–73	0 11.4	−72 06	15.5	0.34	136	0.2	1132
2819	54–5	2 55.8	−70 34	14.0	0.67	98	0.1	609
2890	127–73	3 10.4	−62 28	15.2	0.39	84	0.1	1132
4729	97–12	7 52.8	−67 38	15.0	2.05	135	0.4	602
4834	97–3	8 06.4	−66 09	13.6	0.47	128	0.0	926
5639	64–40	9 28.6	−71 20	16.2	0.43	313	0.2	926
7108	145–141	11 42.9	−64 34	12.5	2.68	97	−0.3	602
7567	68–27	12 25.6	−71 13	17.7	1.17	339	1.0	699
7568	68–28	12 25.6	−71 13	15.7	1.17	339	1.1	699
7756	38–80	12 41.3	−79 53	17.4	0.57	308	0.2	712
7855	147–11	12 48.4	−61 02	15.5	0.08	263	−0.1	1132
8163	40–116	13 14.8	−78 09	17.0	0.47	145	0.5	926
8394	106–73	13 34.5	−67 49	16.1	0.54	263	−0.3	699
8436	106–77	13 37.5	−67 55	15.3	0.11	291	0.0	699
8752	106–24	14 06.3	−66 27	15.6	0.06	329	0.0	1134
8795	149–54	14 09.3	−62 03	15.0	0.11	236	0.2	1134
784	19–2	14 25.4	−81 07	13.0	0.45	208	0.0	926
9025	40–109	14 36.9	−78 10	16.6	0.41	276	0.4	1134
9518	72–91	15 24.3	−74 55	16.7	0.44	238	−0.4	926
11534	158–61	18 28.5	−62 01	16.6	0.25	205	0.2	926
11593	44–95	18 34.7	−78 09	15.2	0.33	161	0.0	984
11668	158–53	18 37.6	−61 56	15.2	0.39	226	0.2	984
12743	160–108	19 44.8	−63 07	11.8	0.44	186	0.2	—
1266	24–52	21 05.2	−82 01	14.4	0.37	168	0.2	984
13870	117–117	21 11.0	—67 14	17.3	0.07	171	0.1	984
14525	48–15	21 59.8	−75 28	14.9	0.52	279	0.1	1142
14703	119–34	22 16.2	−65 44	14.8	0.65	161	0.0	699

main sequence. Eggen, however, calls Vega (M= 0.4 AO) a "bright blue dwarf"; on this system an ordinary highly luminous early O star should be called a "bright violet dwarf", though it is more than a thousand times as luminous as a +3.0 KO star which Kuiper calls a "sub-giant". Kuiper calls the stars between the main sequence and the white dwarfs "subdwarfs" or underluminous stars, but such a subdwarf of M = + 5, AO is only 100 times *less* luminous than an ordinary A star of the main sequence but 1,000 to 10,000 times *more* luminous than some white dwarfs of the same color. Up to now it has not been possible to locate and classify all these stars with certainty in the H-R diagram from either color, spectral class, or proper motion observations or a combination of them. Since observations for parallaxes of very faint stars with large telescopes are no longer popular it does not seem likely that the present confusion and chaos in nomenclature will soon resolve itself. Ultimately when matters become clarified, we may well have to resort to such classification as "main-sequence", "semi-degenerate" and "degenerate", and perhaps more, but until such time comes the present writer prefers to adhere to the simple descriptive term proposed by Adams viz., "intermediate" to designate those stars that lie between the main sequence and the white dwarfs, in addition to retaining the terms "main sequence" and "white dwarfs". While this may lack precision it is at least consistent and avoids ambiguity as much as possible.

Conforming to this terminology then, the majority of stars in groups a, b, and c, probably belong to the main sequence, those listed in Tables I and II are probable candidates for white dwarfs, and the remainder of the stars in group d may provisionally be classified as intermediates. Data for them are given in Table III which is arranged in precisely the same manner as Tables I and II and therefore needs no further explanation.

7. The distribution over the sky of all 65 probable white dwarfs found south of declination −60 is shown in figure 1. The more certain cases, i.e., those found on Cordoba plates, and having large proper motions are represented by solid circles (except BPM 7855, 8436, 8752, 8795, and 13870 which have motions of 0″.1 or less and were only found incidentally in the Cordoba search); those found in the present survey are shown as open circles.

There is some suggestion of galactic concentration but actually this may mean no more than that some galactic regions, because of their strong reddening contain a larger proportion of spurious white dwarfs. The greater concentration of dots in the right hand half of the diagram merely reflects the fact that most of the long-exposure plates are concentrated between 11 and 20 hours of right ascension.

8. No great accuracy can be claimed for any phase of the present sur-

vey — it is indeed no more than just that, a rapid survey of the approximate colors of a large number of stars with appreciable proper motion. Whatever later observations may indicate to be the real accuracy — or lack of it — of the present data, the least that may be said for the present survey is that it has narrowed down the search for white dwarfs from an original total number of 16,000 stars to as few as 800 odd probables and possibles.

In conclusion it is a pleasure to express the author's indebtedness to the Rumford Committee of the Academy for having made possible this investigation, and to Dr. Harlow Shapley, director of the Harvard College Observatory not only for his willingness to have this large program added to the already heavy burden of the Harvard Southern Station, but also for his continued interest in it and his placing all the needed facilities of the Harvard Observatory at the disposal of the execution of this research.

Minneapolis, Minn.,
21 July 1951

TABLE III

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
2	10–47	0	04.0	−83	38	14.9	0.170	90	0.4
70	11–3	1	10.9	−80	36	13.7	.337	220	0.7
88	3–18	1	26.2	−86	50	13.7	.080	135	0.6
131	12–17	2	22.3	−81	16	12.8	.035	109	0.7
137	12–5	2	32.4	−80	30	14.7	.099	262	0.5
181	12–33	3	29.1	−82	22	13.7	.044	225	0.4
182	12–49	3	29.0	−83	38	15.4	.050	225	0.6
192	13–54	3	48.7	−83	55	13.9	.066	321	0.4
211	13–12	4	17.7	−81	18	13.6	.295	85	0.7
214	13–30	4	22.8	−82	21	15.0	.056	238	0.7
279	2–180	4	57.	−89	34	15.1	.083	295	0.5
295	14–27	6	34.6	−82	39	14.2	.123	120	0.5
409	15–30	8	00.2	−81	15	15.0	.148	144	0.5
420	15–40	8	12.0	−81	25	15.0	.117	170	0.4
464	16–79	8	54.5	−83	33	12.3	.167	161	0.3
543	5–30	9	42.2	−86	54	15.0	.135	180	0.6
645	18–8	11	48.2	−81	07	14.5	.130	97	0.4
702	39–47	13	08.9	−80	21	15.6	.103	110	0.7
769	19–61	14	15.1	−83	19	15.4	.060	70	0.3
772	19–78	14	18.0	−85	01	13.8	.157	73	0.3
888	7–22	16	27.2	−86	13	13.2	.060	136	0.2
922	21–50	16	58.6	−83	50	14.8	.195	91	0.4
939	21–14	17	07.5	−81	04	14.2	.142	109	0.4
1052	22–25	18	27.6	−80	50	14.4	.210	117	0.5
1087	23–29	18	49.8	−81	52	15.0	.071	357	0.4

BPM	L	R.A. 1950	Dec	m	μ	p	I.C.
		h m	° ′		″	°	
1123	9–36	19 21.0	−85 45	14.1	.045	104	0.5
1138	9–18	19 25.2	−84 47	11.8	.112	80	0.2
1221	9–48	20 46.8	−86 42	14.2	.042	111	0.5
1236	24–49	20 48.6	−81 52	16.0	.153	152	0.7
1261	24–117	21 04.2	−84 27	14.0	.065	86	0.6
1302	24–11	21 29.0	−80 25	15.4	.154	92	0.4
1347	9–43	22 06.4	−86 27	15.7	.044	266	0.7
1352	24–63	22 03.8	−82 08	15.4	.087	117	0.6

For these stars, all south of declination −80°, the direction of the proper motion is referred to the hour circle of 0^h RA passing through the South Pole, for all other stars it is referred to the usual hour circle passing through the star.

BPM	L	R.A. 1950	Dec	m	μ	p	I.C.
1508	26–57	0 03.1	−77 20	14.9	0.111	153	0.5
1531	169–95	05.3	−62 43	15.0	.069	109	0.5
1611	122–67	12.9	−62 39	11.6	.066	9	0.7
1629	122–80	14.2	−63 44	14.2	.060	108	0.7
1650	86–94	15.9	−69 08	17.2	.162	84	0.9
1693	50–53	20.8	−71 39	13.6	.084	121	0.6
1702	86–17	22.4	−65 55	14.8	.132	104	0.6
1719	50–69	24.0	−72 04	13.3	.049	119	0.2
1727	50–84	24.6	−72 23	14.8	.051	53	0.5
1760	26–21	27.8	−76 03	11.9	.400	154	0.5
1824	122–4	34.2	−60 13	14.3	.282	69	0.4
1849	122–55	38.2	−62 15	14.5	.204	161	0.6
1850	50–128	38.2	−73 22	12.8	.059	133	0.6
1858	122–52	39.1	−62 03	13.6	.077	78	0.6
1865	50–90	39.7	−72 29	13.7	.051	170	0.3
1938	87–5	51.5	−65 12	12.1	.143	102	0.6
1995	124–46	1 00.7	−62 07	13.8	.156	85	0.6
2059	87–71	08.7	−68 04	14.0	.071	149	0.5
2081	52–95	11.6	−73 09	13.5	.041	243	0.4
2105	123–15	14.7	−60 33	14.6	.131	94	0.4
2110	123–31	15.2	−61 24	12.4	.079	74	0.4
2111	51–8	15.2	−70 26	12.4	.182	82	0.6
2145	123–83	18.8	−63 48	13.2	.119	237	0.4
2155	124–58	21.0	−62 45	11.0	.062	73	0.5
2194	124–66	26.6	−63 20	10.8	.213	114	0.4
2195	88–62	26.5	−67 30	15.0	.080	70	0.7
2205	124–20	28.8	−61 00	13.4	.078	81	0.6
2207	88–13	28.8	−65 18	11.5	.096	63	0.6
2228	52–42	31.2	−71 56	14.5	.035	128	0.6
2352	88–26	46.1	−66 22	14.0	.048	220	0.7
2443	28–10	57.1	−75 35	13.6	.043	187	0.2
2452	88–36	2 00.8	−66 45	15.0	.065	152	0.4*
2458	125–44	01.8	−62 54	15.6	.130	187	0.7
2482	53–32	04.2	−72 08	13.9	.083	241	0.5
2519	125–4	10.4	−59 55	13.8	.056	37	0.6

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
2523	89–82	2	10.9	−69	17	14.9	.073	84	0.6
2540	126–50		14.0	−63	32	13.5	.059	107	0.6
2568	28–25		16.5	−76	58	14.2	.097	349	0.4
2569	53–25		17.8	−71	34	13.6	.080	129	0.5
2664	28–13		29.1	−75	45	13.3	.200	141	0.3
2697	126–11		35.4	−61	08	12.1	.224	75	0.5
2872	127–3	3	06.4	−60	10	15.1	.106	161	0.7
2885	127–75		09.4	−62	37	15.0	.114	67	0.6
2911	91–39		14.6	−66	15	13.2	.057	156	0.5
3066	91–132		35.6	−68	44	13.6	.091	148	0.4
3206	91–23		51.2	−65	59	13.1	.062	99	0.7
3273	30–31		59.6	−76	59	15.2	.150	4	0.6
3288	56–43	4	04.0	−72	28	12.5	.077	312	0.5
3290	129–30		05.3	−72	10	11.9	.056	28	0.2
3303	130–44		08.0	−62	35	13.8	.043	160	0.4
3307	92–33		07.5	−69	23	11.9	.129	34	0.6
3320	55–41		09.5	−71	57	12.6	.202	70	0.4
3340	92–39		15.5	−68	59	14.4	.142	169	0.7
3357	130–58		19.1	−64	06	12.6	.103	23	0.5
3403	56–41		22.3	−72	21	13.7	.262	88	0.7
3417	56–35		24.5	−72	12	12.8	.314	38	0.6
3418	130–21		25.9	−61	44	13.3	.065	83	0.6
3481	31–67		37.6	−77	45	15.4	.042	204	0.5
3485	93–17		40.6	−66	06	12.5	.114	135	0.4
3492	56–44		40.6	−72	42	13.7	.063	350	0.5*
3518	131–4		49.2	−60	50	13.0	.173	71	0.5
3573	57–27		56.2	−71	24	12.2	.215	28	0.4
3585	31–51		55.8	−77	25	13.3	.082	65	0.5
3588	131–15		58.9	−63	19	13.8	.095	184	0.7
3595	57–50		59.9	−72	33	14.7	.071	191	0.7
3608	93–23	5	03.1	−66	27	12.0	.202	13	0.5
3643	31–81		06.5	−78	10	13.0	.315	36	0.6
3651	57–34		09.6	−71	32	14.7	.075	187	0.7*
3659	132–48		11.8	−62	49	14.4	.056	29	0.6
3735	57–47		22.4	−72	33	14.8	.107	23	0.5
3759	31–18		21.8	−75	57	14.1	.081	108	0.6
3778	58–45		25.6	−71	02	14.8	.031	171	0.4
3780	132–22		26.9	−61	45	14.7	.038	179	0.6
3784	58–24		27.4	−70	30	15.6	.114	38	0.6
3804	132–14		30.7	−61	09	15.0	.105	140	0.7
3831	31–1		32.2	−74	52	13.5	.112	113	0.6
3856	133–57		37.1	−62	10	14.0	.022	183	0.5
3871	133–70		39.1	−62	42	15.5	.027	189	0.5
3957	32–19		47.8	−76	22	15.2	.096	63	0.6
3960	133–56		50.7	−62	09	15.1	.076	163	0.6

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
3989	94–11	5	53.1	−65	40	13.6	0.075	113	0.7
3997	32–22		51.8	−76	39	15.1	.119	355	0.7
4064	58–4	6	00.8	−69	27	12.4	.075	347	0.6
4132	58–89		08.1	−72	12	15.0	.106	24	0.6
4154	59–64		11.9	−74	43	14.4	.023	97	0.6
4166	134–59		16.3	−62	48	13.2	.037	154	0.5
4182	134–92		19.6	−64	51	14.1	.202	88	0.5
4206	59–1		23.1	−69	47	12.9	.155	134	0.4
4222	33–82		23.1	−78	22	13.7	.100	13	0.6
4237	134–8		28.3	−60	17	13.8	.054	346	0.7
4240	59–28		27.0	−72	27	15.0	.250	38	0.7
4242	59–56		27.2	−74	01	14.9	.133	17	0.5
4267	134–53		34.6	−62	23	14.5	.150	161	0.6
4273	134–21		35.8	−61	10	13.3	.037	231	0.4
4291	33–64		35.7	−77	24	11.6	.123	16	0.2
4292	134–14		38.9	−60	40	13.8	.156	50	0.7
4302	59–16		39.4	−71	27	15.5	.038	230	0.3
4324	33–4		45.2	−74	55	13.2	.083	8	0.5
4353	135–20		53.6	−62	22	12.8	.075	4	0.3
4364	59–46		54.5	−73	26	13.9	.164	130	0.6
4383	59–25		58.1	−72	14	13.3	.107	147	0.5
4401	60–53	7	02.0	−74	15	15.3	.106	3	0.7
4406	33–36		02.9	−76	10	15.2	.111	41	0.6
4415	33–66		04.6	−77	46	13.8	.086	16	0.6
4489	33–60		16.2	−77	24	14.4	.094	329	0.6
4504	34–14		18.4	−75	42	13.6	.032	164	0.5
4524	136–10		23.6	−60	42	14.4	.073	145	0.4
4566	136–50		29.3	−62	23	13.8	.077	142	0.7
4587	61–9		31.8	−70	30	14.7	.136	3	0.7
4640	61–46		38.4	−72	25	15.2	.154	54	0.6
4652	33–56		38.8	−76	57	12.2	.314	354	0.3
4663	137–97		42.6	−63	55	14.7	.027	9	0.5
4686	137–64		46.9	−63	02	12.4	.081	359	0.3
4692	34–44		45.4	−77	34	15.2	.115	174	0.7
4699	61–67		47.1	−73	30	14.5	.127	10	0.6
4710	137–37		50.3	−61	57	13.6	.052	300	0.3
4731	61–6		52.7	−70	14	13.6	.029	152	0.6
4740	137–19		54.8	−61	10	12.7	.086	302	0.4
4765	137–78		57.9	−63	34	13.7	.065	145	0.3
4774	97–6		58.9	−66	33	14.6	.177	160	0.6
4789	61–38		59.6	−72	04	14.5	.093	336	0.5
4823	137–39	8	05.7	−62	02	13.6	.101	331	0.4
4864	137–70		10.2	−63	05	14.0	.050	153	0.4
4866	137–101		10.5	−64	00	13.9	.034	293	0.4
4877	98–66		11.4	−68	21	12.8	.070	119	0.6

BPM	L	R.A. 1950	Dec	m	μ	p	I.C.
		h m	° ′		″	°	
4891	61–76	8 14.1	−74 08	14.6	0.080	246	0.7
4900	98–73	16.9	−68 41	13.1	.137	144	0.6
4906	138–24	18.6	−62 26	12.6	.057	358	0.4
4921	138–34	20.6	−63 27	14.2	.040	135	0.7
4932	34–18	20.9	−76 08	12.1	.048	342	0.5
4952	63–25	25.1	−71 24	13.8	.029	224	0.1
4957	63–54	25.5	−72 25	12.8	.103	123	0.2
4964	63–62	26.3	−72 54	12.4	.102	304	0.3
4968	138–21	28.0	−62 04	11.8	.060	290	0.2
4983	34–53	28.7	−77 58	13.4	.090	326	0.6
4991	138–26	33.0	−62 37	13.2	.112	342	0.5
5024	98–52	37.1	−67 53	14.4	.088	345	0.6
5027	62–7	36.9	−71 14	14.1	.033	301	0.7
5088	139–5	47.6	−60 30	13.3	.126	355	0.3
5094	139–7	48.2	−62 23	13.0	.089	301	0.3
5115	62–21	49.3	−72 15	13.8	.147	331	0.4
5191	140–322	9 00.8	−63 37	13.7	.039	108	0.3
5230	140–392	04.5	−64 13	13.6	.039	144	0.5
5233	139–82	05.0	−63 52	12.1	.144	126	0.3
5242	140–437	06.0	−64 54	13.1	.052	342	0.4
5281	**139–67**	10.1	−63 01	12.6	.082	96	0.4
5289	140–420	10.7	−64 40	14.4	.059	312	0.4
5313	140–51	12.4	−60 49	14.5	.039	108	0.4
5325	140–447	13.0	−65 02	13.8	.073	287	0.3
5328	140–448	13.2	−65 06	14.5	.042	115	0.4
5329	140–112	13.4	−61 45	15.2	.071	152	0.6
5331	140–281	13.4	−63 18	15.0	.040	130	0.2
5342	140–44	14.3	−60 46	13.0	.093	284	0.3
5343	140–104	14.7	−61 38	14.9	.045	277	0.5
5344	140–353	14.6	−63 51	15.3	.063	110	0.3
5350	64–63	14.1	−72 14	14.0	.052	315	0.4
5357	140–234	15.2	−62 59	15.2	.177	207	0.3
5385	140–153	16.6	−62 14	15.4	.076	212	0.5
5389	99–8	16.4	−66 59	13.8	.311	333	0.4
5394	140–352	17.1	−63 50	14.9	.043	318	0.4
5405	140–331	17.5	−63 47	12.7	.076	299	0.5
5406	64–9	16.9	−70 15	15.6	.057	180	0.4
5408	140–74	17.7	−61 17	13.2	.052	11	0.5
5418	140–263	18.3	−63 13	15.3	.034	164	0.2
5423	140–351	18.6	−63 55	15.2	.040	29	0.3
5435	140–29	19.2	−60 29	12.9	.093	170	0.4
5439	36–13	17.9	−75 13	14.8	.122	322	0.6
5448	63–85	18.4	−74 43	13.6	.114	326	0.4
5478	140–416	21.3	−64 44	14.4	.041	285	0.4
5488	140–79	22.1	−61 20	15.1	.056	347	0.5

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
5512	36–57	9	22.6	−76	50	14.4	0.073	314	0.5
5530	140–150		24.1	−62	13	14.6	.055	328	0.3
5546	140–407		25.2	−64	33	13.3	.063	306	0.4
5553	140–424		25.4	−64	45	13.8	.044	117	0.4
5575	100–89		25.9	−67	51	16.9	.047	149	0.5
5586	140–197		26.7	−62	36	14.2	.028	313	0.2
5587	100–108		26.3	−68	21	14.9	.082	121	0.5
5589	99–12		26.5	−67	14	12.6	.080	282	0.4
5618	140–228		28.2	−62	58	12.6	.082	278	0.1
5631	141–209		28.8	−64	07	13.8	.082	96	0.6
5660	140–312		30.4	−63	34	12.2	.048	112	0.2
5666	36–19		29.4	−75	44	14.5	.091	327	0.6
5702	140–413		32.5	−64	40	14.9	.086	334	0.4
5739	36–27		33.0	−75	57	15.4	.075	357	0.6
5749	140–163		34.8	−62	18	13.8	.080	296	0.5
5751	100–28		34.5	−66	18	15.8	.078	136	0.5
5758	140–401		35.0	−64	25	14.4	.030	332	0.4
5761	36–126		33.8	−79	31	13.9	.127	346	0.5
5799	140–400		37.6	−64	26	14.0	.160	308	0.4
5809	140–181		38.1	−62	24	13.7	.059	284	0.4
5828	140–324		39.2	−63	42	14.5	.044	294	0.5
5848	140–359		40.7	−63	54	14.0	.026	310	0.4
5871	141–138		41.9	−63	09	13.9	.033	149	0.2
5886	64–12		42.2	−70	39	14.7	.076	289	0.5
5887	140–338		43.1	−63	44	14.0	.078	350	0.4
5890	100–81		42.6	−67	40	17.6	.047	156	0.5
5896	141–231		43.2	−65	09	13.6	.054	296	0.4
5936	64–109		44.5	−73	38	13.2	.125	132	0.4
6024	141–133		50.8	−63	05	14.0	.023	130	0.4
6048	141–220		52.8	−64	45	13.3	.088	220	0.3
6064	141–94		54.1	−62	23	14.5	.058	260	0.5
6075	141–100		54.7	−62	37	13.9	.044	160	0.3
6076	141–124		54.7	−62	55	13.7	.058	314	0.4
6095	141–69		56.1	−61	51	13.6	.051	128	0.4
6105	64–47		56.3	−71	34	13.0	.056	142	0.4
6144	100–96	10	00.2	−68	03	15.6	.034	301	0.3
6146	100–74		00.4	−67	22	15.4	.034	249	0.4
6171	141–225		03.2	−64	50	13.4	.036	297	0.4
6180	64–42		03.7	−71	29	14.2	.156	296	0.5
6181	142–57		04.1	−62	37	13.9	.066	280	0.3
6182	141–116		04.3	−62	44	13.9	.096	307	0.6
6191	141–187		05.5	−63	52	13.6	.037	335	0.4
6193	36–45		04.3	−76	42	15.0	.049	326	0.7
6250	101–14		11.5	−66	05	13.7	.109	308	0.4
6266	142–95		13.8	−63	57	14.1	.036	290	0.4

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
6271	36–35	10	13.1	−76	20	16.0	0.046	308	0.7
6294	65–62		14.6	−73	32	16.8	.086	343	0.6
6304	101–33		18.1	−67	18	16.8	.092	310	0.4
6315	142–14		19.6	−61	31	13.3	.045	295	0.4
6316	65–57		19.1	−73	15	12.3	.135	320	0.2
6317	142–87		19.8	−63	47	13.4	.062	324	0.4
6320	101–29		20.7	−66	56	12.9	.070	16	0.4
6329	101–21		21.5	−66	32	17.2	.054	117	0.5
6386	142–108		27.2	−64	39	14.0	.039	253	0.3
6398	66–49		28.7	−72	32	15.3	.066	312	0.4
6407	36–61		29.7	−77	06	14.6	.434	121	0.5
6429	65–34		33.8	−72	08	14.2	.046	94	0.5
6432	36–89		34.0	−78	12	15.6	.073	109	0.6
6437	66–73		34.5	−73	50	16.9	.078	303	0.4
6446	65–49		35.2	−72	47	13.6	.180	10	0.3
6447	142–96		36.0	−64	10	14.8	.050	115	0.3
6453	37–22		35.9	−77	02	14.9	.085	294	0.4
6476	142–65		40.2	−63	06	14.4	.033	192	0.4
6477	142–94		40.2	−63	57	13.1	.063	50	0.5
6485	101–92		40.6	−69	36	13.0	.132	311	0.3
6491	66–96		40.9	−74	48	15.5	.084	277	0.5
6513	102–106		44.0	−68	10	16.3	.140	315	0.4
6525	143–81		45.4	−63	35	13.9	.083	118	0.2
6530	37–21		45.1	−77	06	14.8	.088	263	0.5
6534	66–41		45.9	−72	26	15.4	.101	334	0.6
6547	102–117		46.9	−68	29	15.3	.064	339	0.3
6586	66–48		51.1	−72	42	17.0	.042	305	0.2
6625	143–102		55.0	−64	55	13.3	.101	91	0.3
6632	143–97		55.8	−64	35	13.8	.125	244	0.3
6635	66–36		55.6	−72	03	13.7	.098	289	0.5
6648	66–71		56.5	−73	56	17.0	.083	93	0.5
6716	37–18	11	04.4	−76	50	14.7	.042	292	0.4
6717	143–93		05.1	−64	16	13.5	.104	220	0.5
6737	102–57		07.4	−67	19	14.8	.093	286	0.5*
6790	66–68		11.6	−73	31	15.4	.100	321	0.3
6809	144–128		13.7	−63	42	14.4	.071	288	0.3
6814	102–42		14.0	−67	02	16.7	.069	246	0.3
6824	102–34		15.0	−66	58	15.5	.067	298	0.2
6842	102–73		17.2	−67	38	15.2	.096	257	0.4
6850	66–88		17.8	−74	29	15.0	.084	261	0.5
6861	144–42		18.9	−61	27	13.7	.090	292	0.4
6877	144–134		19.9	−63	50	15.2	.037	286	0.3
6897	102–94		22.4	−68	00	16.3	.039	230	0.3
6900	102–162		23.1	−69	48	14.1	.087	317	0.2
6905	102–40		22.7	−67	01	14.9	.046	241	0.3

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
6914	66–34	11	24.5	−71	55	15.0	0.099	219	0.4
6959	144–172		29.6	−65	01	13.3	.097	277	0.3
6962	144–61		30.2	−62	02	14.4	.042	351	0.3
7015	145–67		34.7	−62	02	15.8	.072	302	0.5
7019	103–72		35.2	−68	07	15.6	.044	213	0.3
7029	144–114		36.7	−63	33	14.3	.083	293	0.3
7054	103–50		38.7	−66	31	14.0	.109	123	0.2
7078	38–45		40.0	−78	16	16.2	.078	280	0.6
7080	38–20		40.8	−77	06	15.5	.129	293	0.4
7085	145–66		41.4	−62	03	15.1	.079	294	0.3
7090	145–64		41.7	−62	02	16.1	.039	266	0.4
7097	38–42		41.9	−78	07	15.6	.026	284	0.5
7100	103–66		42.5	−67	23	16.1	.089	260	0.5
7110	103–79		43.1	−68	32	14.6	.051	108	0.5
7113	145–149		43.3	−64	48	14.5	.081	109	0.5
7121	103–92		43.6	−69	54	12.5	.130	264	0.4
7122	68–59		43.9	−71	57	15.2	.042	81	0.4
7146	145–116		45.9	−63	35	15.8	.189	277	0.5
7164	38–67		47.9	−79	34	16.5	.186	272	0.5
7176	145–63		49.4	−62	07	16.3	.055	286	0.4
7179	103–17		49.5	−67	43	15.1	.071	320	0.4
7187	145–134		50.3	−64	16	16.5	.082	222	0.6
7196	145–100		51.5	−63	02	16.0	.039	264	0.5
7197	67–44		51.7	−74	22	13.1	.052	84	0.5
7221	103–80		53.6	−68	39	12.6	.078	266	0.3
7225	145–122		54.3	−63	48	16.0	.050	292	0.4
7234	103–76		55.1	−68	17	13.7	.082	263	0.2
7238	145–133		55.5	−64	13	15.3	.084	258	0.3
7259	145–47		57.2	−61	41	16.5	.028	281	0.5
7287	67–18		59.6	−72	03	15.6	.085	259	0.3
7320	68–1	12	02.6	−70	11	15.0	.047	330	0.5
7341	145–23		04.9	−60	53	14.0	.091	289	0.5
7365	146–14		07.4	−61	05	16.0	.022	14	0.4
7378	104–95		09.1	−68	51	15.3	.038	113	0.5
7390	68–68		10.5	−72	27	14.7	.109	248	0.5
7432	104–115		13.6	−69	34	13.6	.110	242	0.3
7443	104–61		14.4	−68	10	13.8	.046	269	0.5
7454	145–174		14.7	−63	13	16.8	.282	82	0.3
7504	146–117		20.1	−64	16	14.8	.067	267	0.2
7505	146–125		20.2	−64	29	17.1	.031	328	0.5
7514	104–53		20.8	−67	47	14.5	.056	278	0.5
7526	68–149		22.5	−74	20	15.7	.044	299	0.4
7561	104–117		25.5	−69	38	15.0	.111	285	0.5
7563	38–43		25.7	−78	17	14.6	.039	192	0.5
7574	146–122		25.8	−64	27	15.7	.035	347	0.4

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
7590	68–168	12	27.7	−74	48	16.2	0.030	226	0.4
7596	104–57		28.1	−68	00	14.5	.047	279	0.4
7661	68–139		33.2	−74	16	13.3	.073	213	0.5
7662	147–71		33.0	−62	26	15.8	.047	65	0.6
7668	146–58		33.6	−62	41	17.5	.055	282	0.3
7683	68–153		35.2	−74	26	13.8	.093	324	0.5
7697	105–66		35.9	−67	22	15.6	.123	164	0.6
7712	68–117		37.3	−73	40	15.4	.066	259	0.6
7714	69–85		37.6	−72	59	14.7	.072	270	0.5
7726	104–124		38.7	−65	55	14.7	.085	285	0.3
7728	104–126		39.0	−67	01	14.6	.068	274	0.6
7739	104–83		40.0	−68	38	12.5	.133	104	0.3
7751	146–38		40.7	−62	08	15.0	.088	250	0.4
7801	147–88		44.2	−62	35	16.7	.071	308	0.6
7812	105–2		45.3	−65	06	13.8	.241	300	0.2
7824	147–133		46.2	−63	16	15.4	.157	275	0.4
7844	147–78		48.0	−62	34	17.0	.105	173	0.3
7847	147–158		48.1	−63	50	15.4	.065	290	0.4
7871	69–75		50.3	−72	53	15.6	.051	327	0.6
7892	147–126		51.4	−63	11	16.1	.030	259	0.3
7911	147–39		53.0	−61	45	16.5	.045	265	0.5
7913	147–187		53.0	−64	21	15.8	.024	310	0.4
7921	39–11		54.6	−76	50	17.1	.235	5	0.7
7924	147–49		54.3	−62	01	17.1	.041	128	0.5
7930	39–35		55.9	−78	28	16.7	.087	249	0.7
7931	147–85		53.2	−62	36	16.8	.087	241	0.6
7935	69–131		55.8	−74	14	15.7	.041	114	0.5
7956	147–18		57.4	−61	23	14.0	.062	284	0.2
7959	147–47		57.5	−61	59	14.3	.046	333	0.3
7976	105–24		58.8	−66	12	14.1	.083	292	0.3
7977	147–5		58.7	−60	51	14.6	.086	147	0.2
7982	147–93		58.9	−62	44	15.7	.227	273	0.4
7983	105–73		59.0	−67	46	14.9	.040	165	0.5
7987	69–116		59.5	−73	37	16.4	.062	295	0.6
8022	147–61	13	01.4	−62	13	15.3	.039	337	0.3
8042	105–44		02.8	−66	48	16.0	.038	270	0.7
8044	148–28		02.8	−61	28	16.2	.067	238	0.5
8054	105–22		02.6	−66	08	14.4	.215	273	0.5
8059	69–46		04.1	−72	19	15.1	.043	265	0.5
8076	147–185		05.8	−64	20	14.9	.066	272	0.4
8097	148–70		08.1	−62	20	15.7	.088	245	0.3
8102	105–40		08.9	−66	43	15.4	.023	264	0.6
8126	69–162		11.5	−74	58	15.6	.070	292	0.5
8129	147–172		11.3	−64	00	14.4	.102	278	0.4
8155	148–69		13.2	−62	21	16.6	.110	256	0.5

BPM	L.	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
8159	69–8	13	13.7	−70	55	12.0	0.180	266	0.4
8205	105–39		17.4	−66	34	16.1	.290	255	0.6
8257	69–157		22.4	−74	47	13.8	.075	274	0.6
8261	69–12		22.5	−71	18	13.4	.161	306	0.3
8274	69–113		23.6	−73	40	14.0	.105	255	0.7
8288	146–116		24.6	−63	52	16.2	.043	240	0.5
8307	105–76		26.4	−68	02	14.6	.215	230	0.6
8333	148–141		28.5	−64	27	12.1	.146	235	0.3
8334	148–151		28.5	−64	53	15.1	.074	269	0.3
8343	105–120		29.8	−69	41	14.6	.108	307	0.5
8363	149–152		31.3	−64	55	13.0	.073	219	0.3
8377	69–105		33.0	−73	24	15.7	.096	291	0.6
8405	148–30		34.8	−61	45	15.2	.048	280	0.4
8435	106–53		37.6	−67	13	16.2	.053	209	0.5
8512	149–93		44.2	−63	12	14.9	.064	254	0.3
8524	70–66		46.2	−73	39	14.6	.071	247	0.5
8543	106–71		47.2	−67	47	17.4	.037	244	0.6
8575	106–51		50.6	−67	12	14.2	.187	93	0.3
8582	40–128		52.3	−78	49	15.7	.066	313	0.7
8588	149–10		51.7	−60	25	16.0	.079	246	0.7
8629	149–117		55.7	−63	58	15.8	.039	251	0.5
8659	107–77		58.6	−67	43	14.3	.068	257	0.4
8663	70–155		59.5	−73	54	15.2	.067	261	0.6
8690	106–50	14	01.3	−67	13	14.4	.080	275	0.5
8709	149–79		02.9	−62	41	17.0	.119	261	0.4
8710	149–9		02.9	−60	24	15.4	.114	229	0.4
8717	149–21		03.4	−60	58	16.8	.037	214	0.5
8730	149–42		04.6	−61	36	15.8	.068	221	0.5
8753	149–33		06.3	−61	22	16.3	.128	253	0.5
8780	70–21		08.9	−71	39	14.1	.074	229	0.5
8796	70–14		09.9	−71	06	13.6	.055	245	0.4
8810	40–117		12.5	−78	31	17.0	.036	265	0.4
8814	107–132		11.7	−70	05	15.2	.052	285	0.3
8831	70–25		12.1	−71	46	14.0	.077	250	0.5
8835	40–110		15.2	−78	21	15.1	.058	257	0.5
8842	71–14		15.2	−71	54	15.2	.097	260	0.5
8844	70–62		15.8	−73	27	13.7	.162	264	0.3
8853	71–57		17.3	−75	03	17.3	.067	261	0.6
8862	149–140		17.2	−64	29	16.6	.089	265	0.3
8876	107–122		18.9	−69	25	16.0	.094	259	0.6
8884	40–40		20.3	−76	40	16.9	.113	296	0.4
8908	71–3		21.9	−70	57	16.4	.031	251	0.3
8920	107–46		23.0	−66	51	16.5	.031	198	0.6
8923	71–32		23.8	−73	07	16.4	.024	155	0.5
8939	40–31		27.7	−76	20	16.5	.117	240	0.6

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
8949	150–37	14	26.4	−62	04	13.8	0.234	237	0.3
8951	107–127		27.1	−69	36	15.6	.091	220	0.6
8972	107–121		29.1	−69	32	16.0	.049	261	0.4
8985	107–45		30.7	−66	48	16.2	.030	224	0.5
8991	150–18		30.8	−61	13	15.1	.060	244	0.2
9006	150–76		32.2	−63	54	16.0	.064	241	0.5
9021	108–92		35.2	−67	48	13.9	.052	245	0.3
9045	71–38		38.8	−73	19	15.8	.088	248	0.5
9053	107–53		39.1	−67	03	14.5	.153	247	0.3
9065	151–120		40.0	−64	34	16.5	.105	258	0.3
9079	72–68		41.6	−73	18	13.9	.094	318	0.4
9096	41–60		44.2	−78	30	14.7	.060	257	0.4
9098	151–42		42.6	−61	58	16.2	.105	280	0.3
9125	150–99		45.4	−64	53	15.1	.145	160	0.4
9127	107–47		45.5	−66	50	15.1	.051	192	0.5
9132	108–115		46.0	−68	24	15.2	.075	102	0.5
9134	151–36		45.7	−61	51	15.4	.100	272	0.4
9142	108–141		47.2	−69	21	14.8	.070	274	0.3
9143	151–62		46.7	−62	36	16.5	.130	270	0.3
9149	41–30		48.7	−77	10	16.4	.020	184	0.4
9162	107–65		48.6	−67	23	14.7	.081	257	0.4
9163	151–21		48.5	−61	18	17.1	.154	233	0.5
9189	151–84		51.4	−63	26	17.2	.036	264	0.5
9206	151–106		52.6	−63	59	15.7	.039	261	0.4
9217	108–20		54.2	−65	40	14.2	.139	232	0.3
9225	151–57		54.6	−62	22	15.3	.184	245	0.4
9228	151–8		55.3	−60	44	17.5	.035	145	0.6
9312	151–122	15	02.4	−64	46	15.0	.126	250	0.4
9335	108–46		05.2	−66	41	14.7	.100	239	0.2
9339	41–58		07.5	−78	36	16.6	.070	333	0.6
9342	72–88		07.1	−74	40	16.0	.070	256	0.5
9344	151–44		06.0	−62	18	17.5	.103	238	0.4
9366	152–113		08.4	−65	02	13.5	.061	193	0.4
9380	108–139		10.3	−69	25	15.1	.113	230	0.5
9381	72–72		11.1	−73	33	17.3	.081	261	0.6
9384	41–86		12.9	−79	48	15.5	.059	301	0.5
9402	151–31		11.5	−61	42	15.5	.071	232	0.3
9418	152–109		13.1	−64	46	14.7	.061	245	0.3
9422	72–21		14.1	−71	39	16.3	.054	320	0.6
9426	108–12		13.8	−65	15	15.7	.062	212	0.3
9436	152–41		14.4	−61	49	16.5	.059	211	0.3
9442	73–48		16.2	−73	29	13.0	.096	230	0.2
9469	152–45		17.6	−62	00	15.4	.053	246	0.4
9491	108–71		21.0	−67	22	14.4	.116	212	0.5
9496	108–52		21.5	−66	49	16.2	.085	222	0.5

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
9542	108–104	15	26.5	−68	13	14.9	0.070	236	0.4
9565	73–7		29.2	−70	34	12.6	.112	197	0.3
9566	108–111		29.0	−68	24	15.0	.154	237	0.5
9599	153–80		34.2	−61	50	15.3	.053	348	0.3
9609	152–77		35.9	−63	02	16.3	.027	181	0.4
9634	152–73		38.2	−62	50	15.5	.091	246	0.4
9646	153–154		39.8	−63	09	15.3	.074	227	0.2
9663	109–5		41.4	−65	44	14.7	.186	239	0.4
9677	153–49		42.3	−61	18	13.4	.106	221	0.4
9678	153–104		42.4	−62	12	16.0	.063	252	0.4
9714	42–51		49.3	−79	17	17.3	.037	291	0.6
9717	109–60		47.0	−68	32	15.8	.154	270	0.3
9737	73–49		50.1	−73	40	12.9	.088	243	0.3
9767	153–63		51.0	−61	24	16.4	.049	211	0.6
9774	153–131		52.0	−62	49	16.0	.083	251	0.5
9783	153–62		52.4	−61	27	14.9	.145	216	0.4
9788	42–40		55.4	−78	35	17.6	.056	200	0.5
9827	109–68		56.1	−68	55	14.9	.054	257	0.4
9861	74–9		59.1	−70	19	13.8	.078	122	0.4
9870	73–25	16	00.1	−72	09	12.9	.124	236	0.3
9879	74–45		00.8	−71	23	13.2	.070	203	0.6
9893	153–58		01.3	−61	28	15.5	.062	261	0.5
9896	73–12		02.3	−71	14	12.4	.458	310	0.4
9912	109–87		03.7	−70	07	14.3	.085	254	0.3
9929	73–11		05.6	−71	11	14.6	.040	258	0.2
9931	153–88		04.6	−61	59	16.4	.043	327	0.5
9937	42–8		07.0	−75	58	15.0	.090	248	0.5
9938	153–163		05.2	−63	24	15.0	.063	264	0.4
9951	153–126		06.1	−62	43	16.6	.107	243	0.3
9967	109–36		08.1	−67	29	14.2	.100	227	0.4
9983	74–171		11.3	−74	27	14.3	.074	210	0.5
9997	153–81		11.4	−61	48	16.0	.146	222	0.4
9999	110–24		11.8	−66	45	15.0	.117	203	0.5
10016	74–135		14.5	−73	31	14.4	.092	234	0.4
10020	74–185		15.1	−74	39	14.3	.061	218	0.5
10027	74–57		15.2	−71	43	12.3	.119	224	0.5
10048	74–184		18.6	−74	46	13.6	.039	207	0.5
10076	42–4		21.5	−75	43	17.1	.039	278	0.4
10112	74–83		22.9	−72	19	14.1	.051	201	0.5
10132	154–110		22.9	−63	10	15.8	.061	234	0.3
10150	74–64		26.0	−71	52	13.8	.040	183	0.9
10152	74–63		26.1	−71	51	14.0	.038	201	0.6
10174	74–8		27.4	−70	23	14.4	.044	221	0.5
10212	154–23		29.8	−61	00	13.5	.073	241	0.3
10234	154–158		31.9	−64	19	14.8	.044	224	0.6

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
10265	74–115	16	36.1	−72	57	12.9	0.074	228	0.5
10291	110–51		37.1	−68	14	13.2	.090	252	0.4
10317	42–53		41.8	−79	17	16.8	.040	231	0.5
10383	74–80		45.6	−72	10	12.8	.076	180	0.4
10390	74–51		45.7	−71	42	13.6	.067	215	0.6
10407	74–139		47.6	−73	33	15.6	.073	231	0.4
10411	74–4		47.1	−70	09	13.2	.098	190	0.4
10435	74–58		50.0	−71	49	13.4	.048	207	0.6
10438	74–128		50.3	−73	14	14.3	.043	182	0.6
10453	111–98		51.0	−69	56	14.4	.098	198	0.3
10463	74–124		52.2	−73	03	13.6	.058	209	0.5
10506	111–55		57.0	−67	32	13.4	.048	179	0.4
10519	155–38		57.7	−61	54	15.1	.042	255	0.3
10550	111–19	17	01.7	−66	14	14.8	.107	206	0.4
10579	75–21		05.5	−71	05	13.2	.084	231	0.5
10590	76–50		08.3	−73	52	13.0	.072	239	0.5
10603	111–10		08.1	−65	42	14.5	.111	223	0.4
10605	111–30		08.6	−66	41	14.5	.038	234	0.4
10611	111–37		09.9	−66	58	14.3	.086	272	0.4
10622	111–63		11.5	−68	11	13.5	.091	200	0.4
10623	111–5		11.3	−65	17	14.4	.042	180	0.5
10632	156–23		12.1	−61	37	14.6	.058	158	0.4
10650	111–59		14.1	−67	56	14.0	.080	203	0.4
10651	75–31		14.7	−71	40	13.0	.064	187	0.5
10658	43–47		16.9	−77	40	17.0	.038	352	0.6
10673	75–67		17.3	−72	55	14.2	.061	193	0.4
10676	75–90		17.6	−73	31	13.0	.042	185	0.5
10706	75–77		20.6	−73	15	14.5	.085	244	0.4
10724	111–32		20.8	−66	44	14.5	.078	250	0.4
10736	156–47		21.7	−62	24	15.5	.080	230	0.5
10789	156–59		27.7	−62	46	15.5	.101	177	0.6
10873	156–87		37.1	−63	30	15.9	.065	210	0.5
10901	112–70		40.0	−67	16	12.7	.098	211	0.4
10928	112–50		42.2	−66	40	11.8	.124	167	0.4
10938	112–117		43.3	−69	01	13.6	.049	148	0.5
10968	112–16		45.8	−65	25	15.2	.072	234	0.5
11004	76–6		50.0	−70	26	13.4	.129	183	0.3
11005	44–62		51.9	−77	01	16.7	.036	160	0.7
11031	112–47		51.8	−66	39	13.6	.051	176	0.5
11045	76–28		53.7	−72	19	14.6	.072	222	0.6
11056	112–77		53.6	−67	28	12.6	.061	125	0.4
11090	157–61		56.8	−62	38	16.2	.075	225	0.6
11115	44–50	18	01.5	−76	41	15.2	.112	307	0.6
11148	44–16		03.9	−75	22	15.8	.078	268	0.5
11193	76–56		06.3	−74	08	14.4	.072	170	0.5

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
11215	44–123	18	10.9	−79	47	16.2	0.113	199	0.5
11235	112–82		09.0	−67	39	13.9	.134	157	0.5
11277	76–23		13.3	−71	47	14.0	.113	191	0.4
11286	113–123		13.7	−69	04	16.2	.042	164	0.6
11308	158–23		14.3	−61	03	14.6	.102	171	0.6
11338	158–47		16.7	−61	46	14.9	.044	147	0.7
11358	113–21		18.6	−65	46	15.9	.066	134	0.6
11387	158–21		19.5	−61	10	16.4	.057	184	0.7
11390	45–60		22.0	−76	07	13.7	.067	196	0.5
11402	45–89		22.7	−77	15	15.0	.065	189	0.5
11493	113–118		26.3	−68	54	16.9	.087	189	0.4
11498	45–134		29.6	−79	01	14.6	.070	202	0.4
11514	158–57		27.6	−61	53	16.6	.084	207	0.6
11528	113–86		28.8	−67	50	17.1	.040	228	0.7
11559	158–100		30.0	−62	59	16.2	.040	275	0.6
11566	77–118		31.8	−72	55	15.4	.174	192	0.4
11612	113–115		33.8	−68	45	15.3	.110	191	0.6
11666	45–99		40.6	−77	53	15.8	.052	264	0.6
11676	113–80		39.2	−67	38	15.0	.125	270	0.6
11681	158–84		38.6	−62	35	13.3	.071	157	0.5
11685	159–137		39.0	−63	43	15.2	.051	109	0.5
11695	114–27		39.9	−65	18	15.5	.047	196	0.6
11723	113–29		42.1	−65	59	16.0	.105	108	0.8
11750	114–325		45.0	−69	43	16.2	.098	182	0.5
11755	77–168		45.9	−73	38	15.0	.171	183	0.7
11765	159–66		45.0	−62	12	16.7	.084	141	0.6
11841	159–134		50.5	−63	39	16.7	.058	206	0.7
11852	114–62		51.0	−66	06	13.5	.103	201	0.5
11862	114–279		51.8	−68	58	16.4	.052	206	0.4
11954	159–60		55.4	−62	07	15.5	.163	196	0.6
11963	77–10		56.6	−70	06	14.7	.147	207	0.6
11975	45–74		58.9	−76	59	16.4	.064	227	0.4
12005	159–163		58.0	−64	42	16.0	.064	166	0.4
12018	159–78		58.5	−62	28	16.7	.036	151	0.7
12024	159–139		58.9	−63	49	15.3	.082	198	0.3
12026	45–35	19	00.9	−75	41	12.9	.079	173	0.6
12072	114–101		02.3	−66	50	16.0	.052	181	0.7
12085	45–110		05.7	−78	09	16.0	.051	202	0.6
12109	114–68		04.3	−66	16	16.0	.045	163	0.4
12119	114–218		04.9	−68	21	16.7	.025	203	0.6
12190	114–257		09.2	−68	41	17.2	.051	121	0.6
12216	78–63		10.5	−72	33	13.9	.057	205	0.5
12226	160–46		10.4	−61	16	16.4	.073	205	0.5
12228	160–156		10.7	−64	16	15.5	.075	173	0.3
12254	114–66		12.3	−66	18	16.7	.059	218	0.6

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
12256	79–78	19	13.3	−72	53	14.6	0.121	191	0.6
12262	114–157		12.7	−67	40	17.0	.050	163	0.4
12269	160–128		12.5	−63	24	15.8	.030	203	0.4
12271	114–224		13.2	−68	25	16.8	.071	178	0.6
12274	45–108		15.6	−77	55	14.2	.139	200	0.4
12289	159–68		13.5	−62	20	15.2	.046	211	0.6
12296	114–32		13.9	−65	47	15.5	.119	183	0.4
12332	160–127		16.3	−63	26	16.6	.047	224	0.6
12346	160–36		17.0	−60	54	15.7	.073	155	0.3
12388	79–67		20.9	−72	34	14.0	.075	225	0.6
12410	114–31		21.5	−65	41	14.0	.238	178	0.5
12436	79–112		25.0	−74	18	13.7	.053	295	0.5
12450	114–252		24.7	−68	40	12.8	.107	157	0.4
12458	160–84		24.5	−62	29	17.1	.076	144	0.5
12459	114–312		25.3	−69	34	15.8	.073	175	0.6
12474	114–262		26.3	−68	44	16.3	.078	118	0.6
12486	160–15		26.4	−60	25	17.3	.038	227	0.6
12502	78–73		29.0	−72	47	14.2	.073	258	0.6
12508	79–17		29.0	−70	54	13.6	.050	147	0.6
12540	160–7		30.2	−60	17	16.0	.155	193	0.6
12548	45–113		33.6	−77	54	12.6	.142	156	0.5
12549	79–107		32.4	−74	05	14.8	.104	222	0.5
12566	160–101		32.3	−63	01	16.4	.052	184	0.6
12579	160–37		33.3	−61	04	15.1	.134	157	0.7
12585	45–31		34.9	−74	54	15.2	.073	160	0.6
12599	79–8		35.6	−70	20	14.2	.062	172	0.4
12637	160–53		37.4	−61	38	17.9	.105	205	0.7
12655	46–59		41.1	−75	53	15.4	.085	210	0.6
12662	160–160		40.0	−64	47	15.2	.174	158	0.6
12667	160–18		40.0	−60	30	16.3	.141	140	0.5
12671	79–81		41.9	−73	16	15.1	.108	174	0.7
12704	160–133		42.4	−63	40	16.3	.115	231	0.7
12712	160–122		42.8	−63	23	17.4	.096	213	0.6
12720	115–100		43.8	−67	17	15.6	.063	166	0.7
12765	115–61		47.4	−66	34	17.7	.081	220	0.6
12809	115–153		50.8	−68	17	17.7	.050	204	0.7
12830	161–26		51.1	−61	59	17.6	.080	197	0.7
12832	161–13		51.2	−60	51	17.2	.110	194	0.7
12837	115–77		52.1	−66	53	17.7	.045	212	0.7
12839	79–61		53.1	−72	26	15.8	.062	136	0.4
12849	46–157		55.5	−78	37	14.0	.097	195	0.5
12885	79–93		57.4	−73	46	12.6	.166	164	0.4
12915	79–37	20	00.3	−71	35	14.8	.055	170	0.6
12916	79–109		01.0	−74	19	14.2	.164	216	0.5
12922	162–169		00.0	−63	27	13.5	.058	155	0.6

BPM	L	R.A. 1950	Dec	m	μ	p	I.C.
		h m	° ′		″	°	
12931	46–122	20 02.7	−77 31	15.8	0.084	215	0.4
12946	162–87	01.4	−62 19	11.7	.157	189	0.3
12958	115–124	02.5	−67 44	17.0	.110	191	0.6
12968	80–150	04.4	−73 44	16.1	.067	219	0.6
12979	80–149	05.4	−73 46	16.6	.080	178	0.4
12997	162–149	05.8	−63 15	16.2	.099	164	0.7
13000	116–111	06.5	−69 41	16.6	.070	229	0.7
13004	79–86	07.4	−73 22	12.2	.262	191	0.5
13007	80–178	08.2	−74 29	16.7	.054	162	0.7
13010	46–7	08.7	−74 29	14.4	.092	169	0.7
13050	46–150	14.2	−78 16	16.0	.289	215	0.6
13061	162–77	12.4	−62 12	15.6	.110	221	0.5
13114	116–22	16.1	−65 44	13.9	.130	176	0.6
13118	80–11	17.1	−70 20	15.8	.076	152	0.6
13140	46–53	19.3	−75 53	15.5	.055	173	0.5
13142	116–11	17.7	−65 21	15.4	.188	194	0.5
13146	162–106	17.7	−62 40	16.8	.074	162	0.7
13167	162–22	19.0	−60 47	16.3	.054	195	0.6
13182	80–104	21.4	−72 53	17.3	.051	209	0.7
13199	80–6	21.7	−70 01	15.0	.066	127	0.4
13235	162–127	22.8	−63 02	18.0	.076	159	0.7
13239	46–102	24.6	−77 04	14.2	.163	168	0.6
13263	162–137	24.6	−63 15	17.5	.062	196	0.7
13275	162–229	25.4	−64 45	16.6	.189	96	0.3
13325	80–41	29.7	−71 22	16.3	.052	119	0.5
13367	80–31	32.6	−71 07	16.7	.053	231	0.7
13377	80–102	32.5	−72 49	16.9	.100	185	0.7
13383	80–121	34.1	−73 23	16.7	.078	223	0.5
13390	80–30	34.2	−71 04	15.9	.052	175	0.5
13410	162–14	34.5	−60 39	16.7	.209	181	0.6
13439	162–44	35.8	−61 32	16.9	.067	174	0.7
13447	80–101	37.3	−72 51	15.0	.094	147	0.6
13459	162–56	36.9	−61 52	15.0	.110	177	0.6
13469	162–189	37.4	−63 55	16.2	.065	190	0.4
13502	116–123	40.8	−69 43	15.5	.065	173	0.6
13524	80–92	42.8	−72 29	17.6	.089	209	0.5
13531	47–106	45.0	−79 01	12.8	.120	158	0.4
13532	162–212	42.4	−64 26	15.2	.095	103	0.5
13555	117–132	45.0	−67 52	16.7	.060	124	0.7
13591	80–28	48.1	−70 57	13.1	.074	298	0.6
13616	117–44	49.1	−66 20	16.5	.041	170	0.7
13650	163–28	51.9	−60 51	15.0	.123	211	0.5
13664	163–82	53.4	−62 39	15.9	.089	165	0.7
13682	117–89	55.2	−67 19	13.7	.062	146	0.6
13696	117–127	56.8	−67 58	17.3	.066	156	0.5

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
13703	81–50	20	57.9	−72	40	13.2	0.175	289	0.5
13739	117–96	21	00.4	−67	22	16.1	.065	232	0.7
13746	117–79		00.7	−67	02	16.8	.071	138	0.6
13760	47–3		02.8	−74	33	14.2	.120	172	0.5
13769	117–111		02.9	−67	42	14.9	.060	147	0.6
13788	117–35		04.3	−66	14	16.9	.045	234	0.6
13789	117–83		04.5	−67	12	16.3	.057	145	0.7
13822	117–110		06.6	−67	36	14.9	.097	146	0.7*
13849	164–121		08.6	−63	06	16.7	.124	158	0.6
13880	117–72		12.1	−66	47	12.1	.198	181	0.5
13888	117–40		12.5	−66	20	17.2	.102	180	0.7
13908	117–102		14.6	−67	28	11.9	.162	260	0.3
13942	164–112		17.0	−62	49	16.2	.054	236	0.6
13955	164–58		17.6	−61	29	15.3	.102	229	0.5
13958	81–29		18.7	−71	23	12.9	.102	92	0.5
13961	164–135		18.2	−63	35	15.7	.086	214	0.5
13962	81–43		19.0	−72	21	14.2	.078	159	0.6
13964	117–70		18.7	−66	44	15.7	.067	163	0.7
13994	47–16		21.8	−75	21	12.4	.095	303	0.4
13999	164–119		20.6	−63	15	16.9	.052	157	0.7
14000	117–163		21.0	−68	36	15.5	.051	210	0.5
14001	164–31		20.6	−60	52	14.5	.099	149	0.6
14002	164–62		20.6	−61	35	15.5	.072	132	0.5
14045	118–94		24.5	−66	29	14.0	.087	170	0.3
14056	164–46		24.9	−61	12	12.6	.066	106	0.5
14059	164–132		25.1	−63	32	15.1	.143	159	0.6
14083	164–49		27.4	−61	20	15.9	.055	216	0.5
14101	81–39		29.4	−71	57	13.6	.143	136	0.6
14102	81–51		29.5	−72	50	13.8	.052	167	0.6
14114	164–33		29.4	−61	01	15.9	.037	157	0.6
14128	164–148		30.9	−64	04	15.3	.063	191	0.6
14210	48–16		38.0	−75	25	12.2	.055	155	0.6
14218	164–101		37.1	−62	41	15.2	.098	118	0.5
14219	164–130		37.1	−63	24	13.1	.086	130	0.5
14268	118–198		41.2	−68	25	16.5	.086	245	0.7
14270	164–137		41.0	−63	37	13.4	.104	148	0.4
14303	165–22		41.9	−61	01	16.0	.068	266	0.6
14315	82–6		44.5	−70	12	14.4	.156	173	0.7
14319	118–181		44.5	−68	03	16.4	.057	248	0.7
14360	82–2		47.5	−70	09	13.7	.060	179	0.6
14377	165–115		48.4	−63	46	16.6	.205	147	0.5
14385	118–195		49.3	−68	22	15.5	.045	179	0.5
14391	118–140		49.5	−67	21	15.5	.046	92	0.3
14398	165–21		50.0	−60	59	15.3	.074	175	0.5
14417	118–75		51.4	−66	19	16.5	.128	84	0.6

BPM	L	R.A. 1950		Dec		m	μ	p	I.C.
		h	m	°	′		″	°	
14441	165–154	21	53.0	−64	44	15.2	0.126	247	0.5
14501	165–64		56.7	−62	27	16.8	.054	240	0.7
14596	83–23	22	05.2	−71	24	14.4	.092	194	0.7
14599	118–143		05.4	−67	22	16.0	.062	127	0.6
14659	48–91		13.4	−78	31	15.9	.081	110	0.7
14698	119–149		15.7	−67	46	15.2	.059	174	0.6
14732	119–227		19.7	−69	46	15.9	.101	133	0.7
14755	119–71		21.3	−66	35	16.4	.064	208	0.4
14772	48–77		23.8	−78	06	15.2	.242	136	0.7
14801	119–70		25.1	−66	33	14.8	.094	345	0.5
14806	119–8		25.6	−65	20	13.5	.101	123	0.6
14890	119–77		31.5	−66	44	15.9	.044	228	0.7
14907	166–68		32.2	−62	13	14.6	.066	120	0.7
14917	119–75		33.3	−66	45	15.1	.065	146	0.6
14955	166–53		35.3	−61	44	15.0	.074	96	0.6
14957	119–53		35.5	−66	12	13.5	.088	223	0.6
14959	49–70		36.6	−77	10	15.7	.097	105	0.6
14978	166–15		37.5	−60	12	12.8	.074	182	0.4
15010	120–151		40.0	−68	31	14.6	.033	280	0.5
15057	84–25		44.0	−71	50	15.2	.147	127	0.7
15059	119–106		44.0	−67	11	15.3	.070	40	0.6
15061	120–108		44.2	−67	41	14.7	.061	205	0.5
15062	167–79		44.1	−61	43	15.3	.027	70	0.5
15081	167–101		46.0	−62	16	15.8	.076	126	0.5
15093	84–39		46.8	−72	15	15.6	.124	172	0.6
15106	49–120		48.6	−78	35	14.8	.057	88	0.5
15123	83–4		49.0	−70	08	14.6	.080	234	0.6
15174	167–44		52.3	−60	57	14.5	.085	87	0.5
15254	167–133		59.2	−63	12	14.6	.124	96	0.7
15265	84–35		59.2	−72	14	17.0	.105	126	0.4
15268	167–163	23	00.1	−64	14	13.7	.108	127	0.4
15269	120–62		00.2	−66	40	13.8	.044	255	0.5
15302	120–32		01.8	−65	56	15.7	.114	68	0.6
15342	167–147		03.6	−63	32	14.8	.042	87	0.6
15371	49–81		08.4	−77	32	14.8	.128	113	0.7
15422	168–23		10.8	−61	04	14.9	.068	142	0.7
15439	167–117		12.0	−62	48	14.6	.077	170	0.6
15449	49–23		12.9	−75	44	16.0	.079	198	0.7
15450	120–111		12.7	−67	55	16.8	.148	164	0.6
15475	121–22		14.3	−67	05	13.9	.151	98	0.7
15482	26–82	23	15.9	−78	08	14.6	.168	198	0.6
15512	168–36		17.0	−62	12	13.4	.042	158	0.5
15519	85–23		18.5	−70	54	12.7	.081	85	0.5
15527	168–53		18.7	−63	22	14.0	.107	129	0.6
15683	85–117		33.8	−73	02	14.4	.056	147	0.5

BPM	L	R.A. 1950	Dec	m	μ	p	I.C.
		h m	° ′		″	°	
15739	85-46	23 38.5	−71 30	14.8	0.099	142	0.7
15741	169-107	38.5	−63 04	15.3	.050	106	0.6
15787	169-55	42.5	−61 41	13.8	.049	110	0.7
15869	26-59	49.8	−77 14	14.6	.070	112	0.7
15892	85-22	52.1	−70 57	13.4	.082	106	0.7

Notes to the Table

2452 For this star a color index of +1.3 was estimated from Cordoba plates (Ap.J.,96:57, 1942) but it now appears that this estimate is based on an erroneous identification of the proper motion star which is actually much bluer.

3492 This star forms a wide pair with BPM 3489, the latter star is red.

3651 This star forms a wide pair with BPM 3645, the latter star is red.

6737 This star forms a wide pair with BPM 6741 which appears to have a color index of around +1.0.

13822 This star forms a wide pair with BPM 13826, the latter is red.

The Problem of Underdeveloped Areas in Asia and Africa

FRANCIS B. SAYRE
U. S. Representative in U. N. Trusteeship Council

Vol. 81, No. 6
Proceedings of the American Academy of Arts and Sciences

THE PROBLEM OF UNDERDEVELOPED AREAS IN ASIA AND AFRICA*

FRANCIS B. SAYRE
U. S. Representative in U. N. Trusteeship Council

My topic this evening is a world problem, as tremendous in its proportions, as profound in its far-reaching consequences, as the stupendous struggle now raging between Soviet Russia and the free world. It is the problem of the world's far-flung underdeveloped areas. It affects directly the question of world peace or war in the twenty-first century. Upon its successful solution hangs the further advance of civilization.

I

Just what is this problem?

It is the perilous situation resulting today in large parts of Asia and in most of Africa from the conjunction of three poison-breeding factors:

First. A condition of appalling human need. It is not that the peoples in large parts of Asia and Africa lack merely the good things of life. They are born into a lifelong struggle against desperate hunger, against disease that saps their strength, against ignorance and illiteracy that shuts out hope. Living standards in most of Asia and Africa are the lowest in the world. In many sections, life expectancy at birth is only 32 years. One out of every three babies dies before reaching its first birthday. Those suffering from malaria in Asia today equal the total population of the Western Hemisphere — and every year three million of these sufferers die. Tuberculosis, malaria, and yaws are rampant. All are controllable diseases. Monstrous illiteracy bars the door to spiritual or technological advance. More people in Asia and Africa are unable to read a word from a printed book or direction than inhabit the whole of Europe and of the United States.

Second. Embittering memories of cruel racial discrimination and exploitation which accompanied much of nineteenth-century colonialism. These have left livid scars. Racial inferiority complexes have been generated; and these today offer serious hindrances to Western attempts to build bulwarks for freedom. The poison of racial hatred has bred among many people in Asia and Africa profound distrust, and in some cases fear, of all white peoples.

---

*Communication presented before the Academy at its meeting on April 9, 1952.

Third. Surging forces of nationalism. Earlier conditions which made for isolation of underdeveloped peoples have been largely swept away by modern commerce, the radio and world-scale military activities. In consequence, Asia and Africa today, emerging from the primitive conditions which have locked them in for centuries, are being confronted with twentieth-century problems which they scarcely understand and for which they are quite unprepared. The result is the high explosive of nationalism. Even nations which have recently achieved independent statehood are often rabidly nationalistic. Many of these peoples, freed from the fetters of colonialism and awakening to the world around them, are beginning to feel the striking disparity between the peoples of the Western world, in their eyes luxuriating in plenty, and themselves, lacking even the bare essentials of existence. At the same time, they, like the populace of France in 1789, are tasting new-found power. For, however successfully it may be curbed for a time by political institutions or popular ignorance, power ultimately rests in the hands of the people. Millions of men and women living in the underdeveloped areas of Asia and Africa, more and more insistently are asking why they should live as the disinherited people of the world.

It is the conjunction of these three interrelated factors — desperate human need, the feeling of resentment bred by long years of cruel racial discrimination, and an explosive, new-found nationalism — that constitutes the problem of underdeveloped peoples in Asia and Africa. Here is a problem which must be comprehended and met today if we are to have world peace in the twenty-first century. Even if the Soviet state collapsed tomorrow, there could be no assurance of world peace until this problem is mastered. For peace depends inescapably upon human freedom; and in the face of desperate hunger and need, of deep-rooted racial hatreds, of the sudden acquisition of power by peoples quite unprepared for its responsibilities, there can be no genuine freedom.

II

Significant surface tide-rips now and again reveal these strong, deep-moving currents at work. At Paris last November and December in the General Assembly, one felt their power in the debates of the Fourth Committee, where the representatives of some sixty nations deal with problems of trusteeship and dependent peoples. Of these sixty nations, eight administer the territories of dependent peoples and are responsible for their governance. Some fifty do not carry such responsibility, although within the borders of many of them live primitive peoples whose life is much the same as that of peoples in non-self-governing areas. Consequently, in the Fourth Committee, in spite of support from some non-administering delegations, the eight administering powers can be hopelessly outvoted.

The issues underlying the current Fourth Committee debates are basic. Certain peoples, isolated from the busy pathways of mankind, are today underdeveloped and lack the modern resources, the training and the experience to govern themselves competently or to defend themselves against possible attack by aggressor states.

Leading Western nations during past centuries with or without right have entered the territories of many of these peoples and successfully exercised control and government over them. Under the system of national sovereignty as developed in international law they consider today their right to control these people and to exercise sovereignty over them legally and constitutionally unassailable.

But during the last hundred years throughout the world has come an awakening social consciousness and a deepening sense of the sanctity of those human rights which must lie at the foundation of every lasting world order. As a result men and women everywhere today are questioning the right of one people to govern and control another against the latter's consent; and the challenge is being pressed not only by dependent peoples but also by many other nations which themselves possess no colonial territories. However strongly entrenched in law and in constitutional theory may be the colonial powers' right to rule an alien people, there is a growing popular tendency today to shift the issue from constitutional to moral considerations. The resulting pressures are intensified by old resentments against nineteenth-century colonialism.

The march towards political independence within recent years has been assuming dramatic proportions. Moved by a complex of motives and forces, hastened by the pressures of world opinion, the great colonial powers today in numerous instances are giving up former possessions or putting a time-limit on the continuance of their rule. Since the Second World War some 500 million people — a fifth of the entire population of the world — have won political independence.

But with independence come new problems; and genuine freedom is not to be had until a way can be found to solve them.

Examples abound. Libya as a result of the vote of the General Assembly in 1949 has now become a ''united, independent and sovereign State,'' and the occupying powers, Great Britain and France, have transferred all their governmental powers to the new Libyan Government as from December 24, 1951. Free and democratic national elections have already been held there and a new constitution has been inaugurated.

But independence carries with it inescapable responsibilities. Defense involves large outlays of money; so do necessary buildings and public works. So do adequate educational programs and public health measures. So do schools and hospitals and training institutions for indigenous school teachers and doctors and nurses. Thus far the necessary revenues have for the most part come out of the treasuries of Great Britain and of France, the administering powers. Libya itself lacks sufficient revenues. The United Nations budget is not large enough to support the necessary expenditures. For the time being, Great Britain and France have promised to make good the deficits in the Libyan budget. During the current year the United Nations is advancing to Libya over a million dollars in technical assistance. Similarly, the United States is advancing about $1,500,000 in technical assistance. But the question still remains: how will the Libyan people in the long run meet the necessary costs of economic and social and educational advancement? From where will the money come?

Or take another example. The people of the former Italian colony of Somaliland in 1950 were placed under Italian administration in accordance with the vote of the General Assembly in 1949 and were promised their political independence at the end of ten years. The civil expenditures in Somaliland for the past year were almost double the amount of receipts derived from the Territory itself. Local receipts totalled $4,616,850, while civil expenditures amounted to $8,463,140. The difference is made up by a direct contribution from the Administering Authority. The Government of Italy also assumed all obligations relating to the Security Corps. Also, it is worth noting that about 75 per cent of the direct and indirect taxes in Somaliland are paid by Italians.

Experts express doubt whether Somaliland can ever be a viable state, with a high or even moderate level of government services. They question whether the country possesses sufficient natural resources or possibility of industrial development ever to produce the revenues necessary for an adequately governed self-sufficient state. In 1960, Italy, the administering power, steps out. What then?

Problems such as these face us today in many similar areas. Men and women are questioning the right of any nation to govern an alien people against their will. But the maintenance of independence and the development of economic and industrial resources cost money and require trained personnel. Where are they to come from? Surely the answer is not simple abandonment. Underdeveloped peoples cannot be left to live on in ignorance and want. Human progress imperatively demands that they be given a helping hand. In many of the underdeveloped areas in Asia and Africa man has today perhaps his last opportunity to meet these extended problems with humane and Christian solutions. If he fails, can he be surprised if communism moves in?

Underlying the debates in the Fourth Committee is the effort on the part of many of the non-administering powers to widen the scope of international accountability for the government of dependent peoples beyond the point specifically agreed to by the eight administering powers in 1945 when the Charter was written and the international trusteeship system set up. How far can a system of international accountability for the government of dependent peoples be pushed? In other words, have the representatives of the 60 nations which are members of the United Nations the power to require the administering states to adopt such specific policies in the government of their dependent peoples as the Fourth Committee may decide upon by a majority vote?

The contest takes many different forms. Last November in the opening days of the session, the Fourth Committee voted to grant hearings to representatives of the Ewe people, dwelling within the trust territories of French and British Togoland in West Africa. Since these representatives were invited to present their views in a controversy involving an international trust territory, there seemed little question as to the Fourth Committee's competence to grant the hearings. In fact, the United Kingdom representative at the very outset spoke in favor of the invitation. This, however, was followed by a resolution similarly to grant a hearing to chiefs of the Herero, Nama, and Damara tribes dwelling in South West Africa. South West Africa is not a trust territory but a mandated territory set up by the League of Nations under the administration of the South

African Union. The representative of the Union Government strongly protested that since South West Africa is not a trust territory, such a resolution would be an unconstitutional intervention in their domestic affairs and thus a violation of their Charter rights. Others took a contrary view. Upon the passing of the vote the Union delegation promptly withdrew from the Fourth Committee and boycotted its further proceedings.

There followed severe criticism by the representative of Guatemala upon British rule in British Honduras; and this in turn was followed by critical references by Greece to British rule in Cyprus and by Yemen to British administration in Aden. After a strong British protest there followed an attempt on the part of the Arab group to question French rule in Morocco. French Morocco is neither a trust territory nor under mandate. The representative of Iraq charged that the "lamentable position of Morocco" was basically due to the policy of colonialism pursued by France there. Every manifestation of nationalism, he asserted, was being harshly and sternly suppressed. The French representative, protesting the illegality of the proceeding, and asserting that he could not continue to take part in a debate which was wholly unconstitutional, walked out of the Committee.

In debating the legality of the proceedings most of the non-administering powers took the position that the Fourth Committee was entirely competent to discuss political matters and political aspects not only in trust territories but in all non-self-governing territories as well, and a resolution to that effect was introduced. Running through many minds was the position strongly taken by certain non-administering powers the preceding year, that the Fourth Committee is competent by its vote to determine the specific policies which "the administering authorities are under a clear obligation to implement." Dark clouds began to gather over the Fourth Committee.

In the end, however, the Iraq representative agreed not to press the matter to a vote, and the French representative returned to the Fourth Committee. But none of us could fail to realize the deep-seated cleavages becoming manifest. Intense emotions had been aroused; far-reaching issues were at stake.

One must not over-magnify the seriousness of these incidents of last November. On the other hand, every flier must know the air currents upon which he depends. Storm signals are appearing in many quarters. It is not wise to ignore them. Failure to recognize and meet the problem of underdeveloped peoples in

Asia and Africa can seriously injure the machinery of the United Nations, might seriously impair our chances for world peace in the years to come.

III

We must not become confused in our thinking. The problem of underdeveloped areas is not altogether the same as that of dependent peoples. Many dependent peoples, such for instance as those of Bermuda or Malta or Hawaii, possess a comparatively high degree of development, but prefer or need the continuing protection and assistance of a Great Power; whereas, on the other hand, some peoples, such as those in Libya, now possess political independence but remain as yet for the most part underdeveloped. Nevertheless, these two problems of underdevelopment and political dependency are intimately interrelated; in many areas of Asia and Africa they directly coincide. Both reach deep into the heart of the problem of world peace.

World peace, as all of us know, can be built only upon human freedom. Yet today some 200,000,000 people are non-self-governing. What is the solution?

The easy but superficial answer is prompt independence for all. This is the answer which the Soviets beguilingly espoused at San Francisco when the United Nations Charter was being framed in 1945. It is an answer that has instant emotional appeal to almost everyone. It wins support in every General Assembly — particularly among the Latin American groups and those Middle Eastern states which have achieved their independence after long years of struggle.

We of America, perhaps more than any other people, believe that freedom is the rock upon which all human progress must be built. Without it there can be no democracy, no stable world peace.

Under its Charter, the United Nations is consecrated to the task of assisting all non-self-governing peoples in their progressive development towards independence or self-government. This is the deep purpose of every people outside of the Soviet ring.

Since the setting up of the United Nations, as has already been pointed out, some 500 millions of people have acquired political independence. Seven new nations of Asia — India, Pakistan, Ceylon, Burma, the Philippines, the Republic of Korea, and Indonesia — have come into existence. To these must be added Syria, Lebanon, Jordan, and Israel in the Middle East;

and in Africa, Libya, which was given independence last December. Somaliland, Napal, the new states of Indo-China, and others loom on the horizon.

But what so many people fail to understand is that political independence is not synonymous with human freedom. In 1783 the wresting of American independence from the British Crown was only the first step toward freedom. As a next step it took a constitutional bill of rights to guarantee freedom of speech and of the press, freedom of religion, freedom of assembly, freedom from illegal process. Thereafter it took the people of the independent nation many years of sustained effort to build the social and cultural foundations necessary to establish American freedom and make it reasonably secure. Indeed, we are still in the building process. As we move forward we continually gain new vision and new goals.

Genuine freedom cannot be achieved by mere political grant or military victory. It comes only as adequate foundations — political, economic, social, and educational — can be prepared for it.

In Somaliland, for instance, unless the people can through actual experience and training learn what majority rule by secret ballot means and accept the responsibilities that must go with democracy, unless the local revenues can be increased through rapid agricultural or industrial development to pay for sorely needed schools and teachers and hospitals and doctors, there will be no genuine individual human freedom there ten years from now.

Political independence is a notable step along the way. But surely it is only a step, and in no sense the goal itself. Peoples can be as effectively manacled by economic and social forms of servitude as by political oppression. In other words, among the peoples living in many primitive parts of Asia and Africa the real problems go far deeper than political status. In such areas genuine solutions can come only through slow processes of education and training in the fundamentals upon which successful self-government must be built.

When the United States undertook the administration of the Philippine Islands in 1898, in spite of insistent Filipino demands, we did not give them independence for almost half a century. Instead we sent among them armies of school teachers and doctors and road builders. We helped them to learn what democracy means in action and we gave them practical experience in the exasperating art of self-government.

The grant of premature political independence without adequate economic and social preparation for it can bring to a people untold harm. Indigenous leaders, unrestrained by the civic standards that come with widespread education, can exploit their compatriots as ruthlessly as aliens or even more so. Neither can the cause of international peace be served by giving full independence to a people unable to defend themselves. Large parts of Asia and Africa today possess immense natural resources and offer exceedingly valuable strategic bases, but are inhabited by peoples quite unable to hold their own against lawless aggressors armed with twentieth-century weapons.

Our course is clear. If we are to have lasting peace, we must stimulate and help the peoples in all underdeveloped areas, self-governing as well as non-self-governing, to construct the kind of economic and social and educational foundations necessary to prepare them for maintaining their political freedom and to qualify them for increasing self-government.

In this great task the United Nations is now engaged on a broad front through its principal organs and specialized agencies. These, for example, are the precise objectives of the Trusteeship Council for the trust territories of Africa and the Pacific. And it is along these lines in fact that significant and promising advances are actually being achieved today.

IV

Of the six principal United Nations organs it is the Trusteeship Council which devotes its attention most directly to the struggle for human freedom in areas not yet prepared for full political independence.

Today eleven trust territories—seven in Africa and four in the Pacific — have been placed under the international trusteeship system. They embrace a population of some 18,000,000 people. It is true that only about a tenth of the non-self-governing peoples of the world live within the confines of trust territories. Nevertheless, among them the Trusteeship Council has a unique opportunity to point the pathway which leads to freedom for all.

I wish there were time to tell you something of the adventures of the Trusteeship Council along these pathways. I think of the people living in the trust territory of Western Samoa and of how in 1947 their chiefs and leaders sent a petition to the Trusteeship Council asking for self-government. In response the Trusteeship Council promptly sent a Mission to Samoa, composed of a Belgian, a Chilean, and myself. Assisting us were a Norwegian, a Frenchman, and a Brazilian, members of the Secretariat. We spent the summer in Samoa, studying the problem at first hand, visiting the Samoan villages, talking with the people. The Mission reached unanimous conclusions. We recommended that immediate steps be taken to give Samoans a substantially greater measure of self-government. "Even a limited degree of self-government," declared our Report, "involves risks which are not underestimated by the Mission. But these risks must be taken. Training in self-government can come only through actual experience, sometimes costly." We recommended among other things that a Government of Western Samoa should be established and that legislative power should be placed in the hands of a local legislature. We also recommended that Samoans should have an absolute majority in the legislature. Before the year was out the New Zealand Parliament had given to the Samoans a new government embodying substantially every one of our Mission's recommendations.

It takes a visit to a trust territory to see what trusteeship really means. An inspection trip took me not long ago to the former Japanese islands of the Pacific, now a trust territory under U. S. administration. I went out questioning. I came back impressed.

During the period of the Japanese mandate, the peoples of the Islands had been given no training, no hope. War had hit the Islands hard. During my visit, I found that within the two and a half years since the United States had assumed its trusteeship responsibilities more than one hundred local municipalities had been organized to give the inhabitants training in the ways of democratic government. Approximately 80 per cent of the Islanders of voting age at present enjoy some form of suffrage. Almost every inhabited island today has its elementary schools. More than 90 per cent of the children of school age are enrolled in local schools. Indigenous teachers are being taught in an effective training school at Truk, where once the Japanese were building their war machine. Disease has been drastically reduced. Malnutrition has been virtually eliminated. Indigenous medical assistants, dentists, and nurses, trained at Guam or at Suva, are carrying their ministrations throughout the Islands.

Americans are winning a place in the hearts of the Pacific Islanders. Last year came a petition to the United Nations from Saipan, one of the Pacific Islands. ''It is our fervent hope,'' reads the petition, ''that all of the islands in the Northern Marianas be incorporated into the United States of America, either as a possession or as a territory, preferably as a territory.'' To their great disappointment the petition was not granted.

Throughout the trust territories, the inhabitants are learning in the crucible of experience what self-government and freedom really mean.

''But why force upon peoples who through the centuries have developed their own cultures and found happiness in them, a twentieth-century culture which they neither understand nor want?'' many will ask. ''Would it not make for the happiness of all to leave them unmolested in their own ways of life?''

The answer is that we have no choice in the matter. No one can stay the hand of advancing cultures — least of all in an age when insistent commercial and military demands have knit all peoples into an inescapable unity. Western Samoa during the whole of the second half of the nineteenth century struggled unyieldingly to preserve its indigenous culture and its isolation from Western civilization. It was of no avail. Other attempts all tell the same story. In our present shrunken twentieth-century world no people can successfully isolate their indigenous culture behind a Chinese Wall. Each people has incalculable contributions to be given to humanity. Our twentieth-century life demands that every people make its peculiar contributions and that we together share our differing cultures.

V

Once we know the direction in which to work, we must not be dismayed by the magnitude of the task. The exciting fact is that today this problem of underdeveloped areas is being intelligently attacked as never before. The attack is on an international scale, in the main by the United Nations, but also by the coordinated efforts of individual states.

Food is one of the paramount issues. As Josue de Castro has aptly said: ''Only by raising the buying power and consuming capacity of the undernourished two-thirds of the world can the other third survive and prosper. Isolationism can be as dangerous in the matter of food consumption as in the realm of war and politics.''

The UN Food and Agricultural Organization (FAO) is laboring manfully to increase food supplies throughout the world. It is showing many peoples the superiority of the plow to the hoe, of modern scientific agriculture to archaic methods of tilling the fields. Even so, progress is slow. Two months ago I was watching Arabs plowing their fields in Northern Morocco. The plows consisted of the twisted branches of trees. Iron plows were beyond the workers' reach.

Power development, flood control, irrigation projects cost money and cannot be put through without capital from the outside. In the past fiscal year (1951), the International Bank for Reconstruction and Development made twenty-one loans for development projects in eleven countries totalling nearly 300 million dollars.

For the first year of an expanded program of technical assistance and economic development, 54 governments pledged to the United Nations Technical Assistance Board over 20 million dollars. Nearly 19 million dollars has been pledged so far for the 1952 program. Up to June 30, 1951, more than five hundred requests had been received from 64 countries and territories. At that date 252 projects had been initiated or approved through agreements with 45 countries providing for 741 experts and 551 fellowships. From further agreements then under negotiation projects requiring the services of 674 experts and 590 fellowships were expected to result.

Separate nations are also engaging in this considerable effort. The United States total program of technical assistance excluding its contribution to the United Nations amounts to more than 200 million dollars. This is part of the appropriation of 1,440 million dollars voted by Congress last October 31 for economic and technical cooperation. The United Kingdom in 1945 passed the Colonial Development and Welfare Act, setting aside 120 million pounds to promote the development and welfare of the colonies. Other administering powers are pushing forward in similar developments.

Difficulties abound. Economic assistance programs cannot be rammed down the throats of undesiring recipients. At least one non-communist country has refused the offer of American technical assistance because of their fear or misunderstanding of our motives. Also, innovations may arouse the opposition of tribal chiefs or the holders of vested rights. Then, again, old-time subsistence economies, easy-going and never exacting, are often of greater appeal than increased revenues. Progress as conceived of in our Western cultures is not coveted by every people, particularly if it involves the abandonment of ancestral ways of life.

The building of economic foundations for freedom clearly involves more than handing out in backward areas dollars or pounds or francs for expenditure. Like all effective work for human progress it requires a deep understanding of the people concerned, tempered with infinite patience and wisdom. It will not succeed unless wrought with an abiding faith in the dignity and worth of every personality, regardless of the color of his skin or his unfamiliarity with formal education. We have still a long, long way to go. Nevertheless, economic progress is being achieved — more rapidly it would seem and on a more extended scale than ever before in human history.

Adequate social foundations are as necessary as economic ones. In the space of less than three years the United Nations has secured the adoption of a Universal Declaration of Human Rights. This is not a treaty, not a law, but a definition of goals. But it has already had an important impact upon the thinking of mankind. The Treaty on Genocide, now ratified by 35 nations, is an attempt to outlaw the practice of mass extermination of peoples.

Basic among social problems is, of course, that of disease. Sick men cannot till their lands. In this field another United Nations agency, the World Health Organization (WHO), is mobilizing attack upon an international scale. The Trusteeship Council is also constantly at work, pioneering in the trust territories in the field of social problems. For instance, in a recent report covering the trust territory of Tanganyika, the former German colony of East Africa, the following problems are listed as having come under the Council's consideration: social welfare and security, population pressure and movement, standard of living, housing, child marriage, immigration, general labor conditions, wage rates, trade unions, labor disputes, labor conventions and legislation, cooperative societies, medical services, hospitals, dispensaries, clinics, and prisons.

In this field perhaps the most profound and baffling problem of all is racial discrimination. One thing we know. Brazen racial discrimination undermines the position of the white man in the world community. Change will come; and it is greatly to the interest of the white race to help to guide rather than to impede that change. In a number of areas the problem is being solved — and successfully solved — along the lines pointed out by courageous missionaries for many years. They call it the pathway of human brotherhood. The peoples of the United Nations at the very outset of the Charter pledge themselves to a "faith in fundamental human rights", and "in the equal rights of men and women and of nations large and small."

As a matter of fact, experience over many years has shown that when colonial administration is based upon the exploitation of human beings, it has bred only difficulties and well-nigh insuperable problems. On the other hand, when colonial administration has come to be based upon the conception of sharing common problems and common fortunes, stable and reasonably satisfactory solutions have generally been reached. One thinks of New Zealanders and Maoris, of Americans and Filipinos. Wise colonial administration recognizes the oneness of the human race.

Another and perhaps the most necessary foundation for human freedom, particularly among primitive and tribal peoples, lies in the field of education. "If a nation expects to be ignorant and free," wrote Thomas Jefferson, "it expects what never was and never will be." In Asia, Africa, and the South Pacific, many peoples still lack any comprehension of what human progress outside their own static cultures means.

In the weird Cargo Cult of the South Pacific, one gains a glimpse of the thought-processes of the primitive mind. Among these peoples runs an unquestioning belief in magic, in the power of the fetish, in unholy spells. Struck with the utterly inexplicable power of foreigners to produce as from the skies inexhaustible supplies of ships and guns and food and kerosene stoves and machine monsters of indescribable power, certain South Pacific peoples under the spell of the Cargo Cult, perhaps touched by a misunderstood conception of sacrifice gained from Christian missions, will take it upon themselves with sudden decision to destroy everything they possess — to tear down their houses, burn their pathetic belongings, chop down their trees and root up their gardens, in a fine gesture of faith and hope that their gods or ancestors will thus be induced to send to them, too, even as to the foreigners, miraculous machines and a wealth of food.

As a general rule in underdeveloped areas there is an intense eagerness for modern schooling rather than resistance to it. In most such areas it is far easier to get children to come to school than to provide sufficient schools and sufficient trained teachers to teach them. One cannot plan an educational program based upon the use of foreign teachers for a population running into millions or even tens of millions. Manifestly there are not enough foreign teachers to be had. Adequate training schools where indigenous teachers can be trained constitute the keystone of any effective program for widespread education. Into these must be gathered the most promising boys and girls of the territory. This all takes time and costs large outlays of money.

There is also the crucial question of what to teach. It will do no good to give to primitive peoples a classical education. People living in primitive or tribal conditions must learn, not higher mathematics or ancient history, but how to make life more meaningful and rewarding for themselves and their fellows and how to improve the conditions under which they live.

VI

The problem of underdeveloped areas in Asia and Africa cannot be solved in a night. It requires endless effort along a hundred different fronts. Its solution can come only through patient, untiring constructive work, and not through mass destruction.

But the great fact is that the free peoples of the world are comprehending ever more clearly the underlying issues and concentrating upon the constructive way forward. Measurable progress is being made.

The fight for freedom is a twofold fight. It involves not only resistance to political oppression but also emancipation of men and women from the shackles of hunger, disease, and ignorance. Both are necessary. Victory in one field contributes to victory in the other.

Soviet communism will not win. Neither authoritarianism nor ruthless dictatorship can ever permanently prevail. Free men working together throughout the world for human freedom possess matchless power. Today they are uniting as never before. Their power is unconquerable.

In the words of Christopher Fry:

Dark and cold we may be, but this
Is no winter now. The frozen misery
Of centuries breaks, cracks, begins to move.
The thunder is the thunder of the flows,
The thaw, the flood, the upstart Spring.
Thank God our time is now, when wrong
Comes up to face us everywhere,
Never to leave us till we take
The longest stride of soul men ever took.

Proceedings of the American Academy of Arts and Sciences

Vol. 81, No. 7, p. 299-327, July, 1952

Summer Number

RECORDS OF MEETINGS
FROM OCTOBER 1951 TO MAY 1952

American Academy of Arts and Sciences

OFFICERS AND COMMITTEES FOR 1952–1953

President

Edwin H. Land

Vice-Presidents

Class I	*Class II*
Jerome C. Hunsaker	Alfred S. Romer
Class III	*Class IV*
Erwin D. Canham	Wallace Goodrich *

Secretary
Henry B. Phillips

Treasurer
Horace S. Ford

Librarian
Ernest H. Huntress

Editor
Taylor Starck

Executive Officer

Ralph W. Burhoe

Councillors

Class I		Class II
Edwin H. Land	*Terms expire 1953*	Francis O. Schmitt
Fred L. Whipple	*Terms expire 1954*	Martin J. Buerger
Harold L. Hazen	*Terms expire 1955*	Kenneth V. Thimann
John T. Edsall	*Terms expire 1956*	Francis M. Rackemann
Class III		**Class IV**
W. Rupert Maclaurin	*Terms expire 1953*	George W. Sherburn
Mark DeW. Howe	*Terms expire 1954*	Willard V. O. Quine
Carl J. Friedrich	*Terms expire 1955*	Joseph Hudnut
Chester M. Alter	*Terms expire 1956*	Hugh Hencken

STANDING COMMITTEES

The House Committee

John B. Wilbur (-1953), *Chairman*

Philippe E. LeCorbeiller (-1954)

Henry R. Viets (-1955)

The Committee on Membership

Edwin H. Land, *Chairman, ex officio*

Class I		Class II
William H. McAdams	*Terms expire 1953*	Alden B. Dawson
Edward M. Purcell	*Terms expire 1954*	James M. Faulkner
Class III		**Class IV**
Zechariah Chafee, Jr.	*Terms expire 1953*	Bartlett H. Hayes
Walter M. Whitehill	*Terms expire 1954*	Mason Hammond

---

* Died June 6, 1952.

The Committee on Meetings

Edwin H. Land, *Chairman, ex officio*
Henry B. Phillips, *Secretary, ex officio*

Class I

Jerome C. Hunsaker, *ex officio*
Bart J. Bok (-1954)

Class II

Alfred S. Romer, *ex officio*
Roy G. Hoskins (-1954)

Class III

Mark DeW. Howe (-1953)
Erwin D. Canham, *ex officio*

Class IV

Henry J. Cadbury (-1953)
Wallace Goodrich,* *ex officio*

The Committee on Finance

Horace S. Ford, *Chairman, ex officio*

Ralph E. Freeman (-1953)
Francis C. Gray (-1953)

Thomas D. Cabot (-1954)
Ralph Lowell (-1954)

The Auditing Committee

Thomas H. Sanders (-1953)

Bishop C. Hunt (-1954)

The Committee on Publication

Taylor Starck, *Chairman, ex officio*

Class I

Philipp G. Frank (-1954)

Class II

Frank M. Carpenter (-1954)

Class III

John E. Burchard (-1953)

Class IV

Wilbur K. Jordan (-1953)

The Permanent Science Fund Committee

Hudson Hoagland (-1953), *Chairman*

Helen M. Cam (-1954)
Gustavus J. Esselen (-1953)
Chester S. Keefer (-1955)

Donald H. Menzel (-1954)
John L. Oncley (-1955)
Reed C. Rollins (-1955)

The Rumford Committee

George R. Harrison (-1953), *Chairman*

Daniel F. Comstock (-1954)
Arthur C. Hardy (-1954)
Joseph H. Keenan (-1954)

Norman F. Ramsey (-1955)
Francis O. Schmitt (-1953)
George Wald (-1955)

---

* Died June 6, 1952.

The C. M. Warren Committee

Edwin R. Gilliland (-1955), *Chairman*

Edwin J. Cohn (-1954)
Robert H. Cole (-1955)
Louis F. Fieser (-1954)
Eugene M. Landis (-1954)
Avery A. Morton (-1953)
Walter C. Schumb (-1953)

The Amory Prize Committee

Edwin B. Wilson, *Chairman*

William B. Castle
Roy G. Hoskins
Gregory Pincus
George W. Thorn
George B. Wislocki
S. Burt Wolbach

The Nominating Committee

Harlow Shapley, *Chairman*

Class I

Julius A. Stratton (-1953)
Charles D. Coryell (-1954)
Harlow Shapley (-1955)

Class II

Paul C. Mangelsdorf (-1953)
Hudson Hoagland (-1954)
Clifford Frondel (-1955)

Class III

Douglass V. Brown (-1953)
Zechariah Chafee, Jr. (-1954)
Robert B. Stewart (-1955)

Class IV

Edward A. Weeks, Jr. (-1953)
Wilbur K. Jordan (-1954)
William F. Twaddell (-1955)

SPECIAL COMMITTEES ACTIVE DURING 1952-1953

Committee for the Conference on Climatic Variations

Harlow Shapley, *Chairman*

Elso S. Barghoorn
Columbus O'D. Iselin
Helmut Landsberg
Hurd C. Willett

Committee for the Conference on Totalitarianism

Carl J. Friedrich, *Chairman*

Erwin D. Canham
Karl W. Deutsch
Merle Fainsod
Alexander Gerschenkron
Harold D. Lasswell

Committee on International Relations

John T. Edsall, *Chairman*

Mark DeW. Howe (resigned May, 1952)
Paul M. Doty
Bart J. Bok (appointed May, 1952)
Taylor Starck
Henry R. Viets

Committee on Planning and Development

Edwin H. Land, *Chairman*

Mason Hammond
Milton E. Lord
Ralph Lowell
George C. Shattuck

Committee on School Science

Francis Bitter, *Chairman*

John E. Burchard
Charles D. Coryell
Mason Hammond
Kenneth V. Thimann

Committee on Science and Values

George Wald, *Chairman*

Philipp Frank
Roy G. Hoskins
Henry A. Murray
Henry B. Phillips
Laszlo Tisza
Robert Ulich

Committee on Policy on Membership and Dues

George R. Harrison, *Chairman*

Chester M. Alter
Horace S. Ford
Harold L. Hazen
Harlow Shapley
Kenneth V. Thimann

Committee on Unity of Science

Phillip Frank, *Chairman*

Percy W. Bridgman
Hudson Hoagland
Charles Morris
Harlow Shapley
B. F. Skinner
S. S. Stevens

RECORDS OF MEETINGS

October 10, 1951 — Stated Meeting

The One Thousand Three Hundred and Fifty-second Meeting of the Academy convened in its House on October 10, 1951, and was called to order by the President at 8:30 P.M.

There were present sixty-five Fellows and ninety-three guests.

The records of the Meeting of May 9 were approved as published.

The Secretary reported that the Council had approved a grant of $500 from the Rumford Fund, and nine grants from the Permanent Science Fund totaling $9,065.

Recently elected Fellows were introduced as follows: Caryl Parker Haskins (III:3) by H. Shapley; Chia-Chiao Lin (I:1) by E. Reissner; Egon Orowan (I:4) by C. R. Soderberg; Norman Foster Ramsey (I:2) by J. H. Van Vleck; John Clark Sheehan (I:3) by A. C. Cope; Nathan Bill Talbot (II:4) by J. H. Means.

The following communication was presented:

David F. Edwards: *Should an Old New England Company, Making Heavy Textile Machinery, Rebuild Its Plants in New England or in the Carolinas?*

The meeting was dissolved at 10:30 P.M.

November 14, 1951 — Stated Meeting

The One Thousand Three Hundred and Fifty-third Meeting of the Academy convened in its House on November 14, 1951, and was called to order by the President at 8:27 P.M.

There were present forty-two Fellows and forty-one guests.

The records of the Meeting of October 10 were read and approved.

Recently elected Fellows were introduced as follows: William Phelps Allis (I:2) by N. H. Frank; Harvey Brooks (I:2) by F. V. Hunt; Filmer Stuart Cuckow Northrop (IV:1) by W. V. O. Quine; William Prager (I:2) by C. R. Adams.

Mr. Joseph H. Keenan, Academy delegate to the celebration of the 500th Anniversary of the University of Glasgow, gave a report of the occasion.

Introduced by Mr. Shapley, Chairman of the Rumford Committee, Herbert E. Ives, the Rumford medalist for 1951, received at the hand of the President the Rumford gold and silver medals for his noteworthy contributions to optics.

The following communication was presented:

Herbert E. Ives: *Adventures with Standing Light Waves.*

The meeting was dissolved at 9:34 P.M.

December 12, 1951 — Stated Meeting

The One Thousand Three Hundred and Fifty-fourth Meeting of the Academy convened in its House on December 12, 1951, and was called to order by the President at 8:30 P.M.

There were present thirty-five Fellows and fifty guests.

The records of the Meeting of November 14 were read and approved.

Recently elected Fellows were introduced as follows: Paul Mead Doty (I:3) by J. L. Oncley; Philippe Emmanuel LeCorbeiller (I:2) by E. L. Chaffee; Abram L. Sachar (III:4) by L. Carmichael; Walter Muir Whitehill (III:4) by K. D. Metcalf.

The following communication was presented:

Professor I. Bernard Cohen: *Benjamin Franklin, a Newtonian Experimental Scientist.*

The meeting was dissolved at 9:50 P.M.

January 9, 1952 — Stated Meeting

The One Thousand Three Hundred and Fifty-fifth Meeting of the Academy convened in its House on January 9, 1952, and was called to order by the President at 8:20 P.M.

There were present forty-three Fellows and twenty-four guests.

The records of the Meeting of December 12, 1951, were read and approved.

Recently elected Fellows were introduced as follows: Elizabeth Sprague Coolidge (IV:4) by Wallace Goodrich; William C. Boyd (I:3) by John T. Edsall.

The following communication was presented:

Mr. James M. Barker: *The Middle East, Geographical Solar Plexus.*

The meeting was dissolved at 10 P.M.

February 13, 1952 — Stated Meeting

The One Thousand Three Hundred and Fifty-sixth Meeting of the Academy convened in its House on February 13, 1952, and was called to order by the President at 8:15 P.M.

There were present fifty-four Fellows.

The records of the Meeting of January 9, 1952, were read and approved.

The following recently elected Fellow was introduced: William M. Rand (III:4) by George B. Kistiakowsky (I:3).

The Secretary reported that the Council, at its meeting this afternoon, recommended amending the Statutes in order to revise the classification of membership, and gave notice that these amendments would be presented for action at the March meeting.

Mr. Taylor Starck (IV:4) reported on the annual meeting of the American Council of Learned Societies.

Messrs. W. G. Constable (IV:4) and Charles D. Coryell (I:3), official delegates, reported on the Third National Conference on UNESCO conducted by the United States National Commission for UNESCO. Messrs. Bartlett Hayes (IV:4) and Bart J. Bok (I:1) reported further, informally.

The remainder of the meeting was devoted to a discussion of Academy policy led by President Land.

The meeting was dissolved at 10:30 P.M.

March 12, 1952 — Stated Meeting

The One Thousand Three Hundred and Fifty-seventh Meeting of the Academy convened in its House on March 12, 1952, and was called to order by the President at 8:20 P.M.

There were present thirty Fellows and fifty-one guests.

The records of the Meeting of February 13, 1952, were read and approved.

Upon motion by Mr. Whittlesey, it was voted to table action on the proposed amendments to the Statutes on the membership classification until another meeting.

Upon recommendation by the Council it was voted that the assessment for the fiscal year beginning April 1, 1952, be $15 for Resident Fellows and $7.50 for Non-Resident Fellows.

Upon recommendation of the Council it was voted that the following appropriations be made for the fiscal year beginning April 1, 1952:

A. From the General Funds:

1.	Salaries, Pensions and Annuities	$15,577	
2.	General Office and Meeting Expense	7,075	
3.	House Maintenance	10,050	
4.	President's Expense	300	
5.	Treasurer's Expense	800	
6.	Committee on International Relations	500	
7.	Committee on School Science	500	
			$34,802

B. From the Restricted Funds:		
1. Amory Fund Committee	$ 100	
2. Permanent Science Fund Committee	40,000	
3. Committee on Publication	7,000	
4. Rumford Fund Committee	5,000	
5. Committee on the Unity of Science	2,271.69	
6. Warren Fund Committee	3,000	
		$57,871.69
		$92,673.69

Honored guests of the Academy, the twenty-five New England winners and honorable mentions of the current National Science Talent Search, were introduced by President Land.

Mr. John Welsh, Chairman of the Committee on School Science, introduced Mr. Elbert C. Weaver, of Phillips Academy, Andover, Massachusetts, the recipient of the Academy's first award for outstanding science teaching in the secondary schools of New England. President Land tendered the award of $100 and a citation recognizing Mr. Weaver's "distinguished service in the classroom and of his nationwide activities for the improvement of science instruction."

The following communication was presented:

George R. Harrison: *The Role of the Secondary School in the Teaching of Science.*

The meeting was dissolved at 9:45 P.M.

April 9, 1952 — Stated Meeting

The One Thousand Three Hundred and Fifty-eighth Meeting of the Academy convened in its House on April 9, 1952, and was called to order by the President at 8:20 P.M.

There were present fifty-five Fellows and twenty-seven guests.

The records of the Meeting of March 12, 1952, were read and approved.

The Secretary reported that the Council had reviewed suggestions for further changes in the proposed reclassification of the membership. The recommendations of the Council for amending the Statutes in connection with the classification will be published in the announcement of the May meeting, at which meeting they will be brought up for action.

Recently elected Fellows were introduced as follows: Arthur Francis Buddington (II:1) by H. E. McKinstry; Yuen Ren Chao (IV:3) by T. Starck; George Frisbie Whicher (IV:4) by G. Scatchard.

The following communication was presented:

Francis B. Sayre: *The Problem of Underdeveloped Areas in Asia and Africa.*

The meeting was dissolved at 10:15 P.M.

May 14, 1952 — Annual Meeting

The One Thousand Three Hundred and Fifty-ninth Meeting of the Academy convened in its House on May 14, 1952, and was called to order by the President at 8:25 P.M.

There were present forty-six Fellows and fifty guests.

The records of the Meeting of April 9 were read and approved.

It was voted to amend the Statutes, as follows:

1. *Amend Chap. II, Art. 1, to read:*

ARTICLE 1. The Academy shall consist of Fellows, elected from the citizens or residents of the United States of America, Fellows Emeriti, and Foreign Honorary Members. They are arranged in four Classes, according to the Arts and Sciences in which they are severally proficient. Each Class shall be divided into the several Sections shown in the following table.

CLASS I. MATHEMATICAL AND PHYSICAL SCIENCES
- Section 1. Mathematics
- 2. Physics
- 3. Chemistry
- 4. Astronomy
- 5. Earth Sciences[1]
- 6. Engineering Sciences and Technologies

CLASS II. BIOLOGICAL SCIENCES
- Section 1. Biophysics and Biochemistry[2]
- 2. Botany and Bacteriology
- 3. Zoology[3]
- 4. Physiology and Experimental Psychology
- 5. Medicine[4]

CLASS III. SOCIAL ARTS AND SCIENCES
- Section 1. Social Relations[5]
- 2. Economics
- 3. Political Science
- 4. Law
- 5. Administration and Affairs[6]

---

[1] Including geology, geography, mineralogy, meteorology, and oceanography, etc.

[2] Including pharmacology, etc.

[3] Including physical anthropology, etc.

[4] Including surgery and psychiatry, etc.

[5] Including human geography and cultural anthropology, clinical and social psychology, etc.

[6] Including diplomacy, etc.

Class IV. Humanities
Section 1. Philosophy and Theology
2. History and Archaeology
3. Philology and Criticism[7]
4. Fine Arts and Belles Lettres

2. *Amend Chap. II, Art. 2, paragraph 1, to read:*

Article 2. The number of Fellows shall not exceed one thousand, of whom not more than two-thirds shall be residents of New England. The number of Fellows in the four Classes, I through IV, shall be approximately in the ratio of 6 : 5 : 5 : 4, respectively.

3. *Amend Chap. IV, Art. 1, paragraph 2, to read:*

There shall be also 20 Councillors, one for each Section of each Class; at each Annual Meeting 5 Councillors shall be elected by ballot to serve for a term of four years, and they shall take office at the close of that meeting and shall hold office until others are duly chosen and take office. The same Fellow shall not be eligible for two successive terms.

The annual reports were presented and accepted. New members and officers and committees were elected. These reports and elections are printed below.

The following communication was presented:

G. Wallace Woodworth, assisted by the Harvard Glee Club and the Radcliffe Choral Society: *Words and Music.*

The meeting was dissolved at 10:15 P.M.

Henry B. Phillips, *Secretary*

ANNUAL REPORTS

1951 — 1952

Report on Deaths, Resignations, and the Status of the Membership

1951-1952

I regret to have to report the following deaths: Twenty-eight Fellows and two Fellows Emeriti—Michael Joseph Ahern, S.J. (IV:1), Edwin Montefiore Borchard (III:2), Joseph Perkins Chamberlain (III:2), George Henry Chase (FE IV:2), Henry Asbury Christian (II:4), Clive Day (III:3), Henry Sturgis Dennison (III:4), John Dickinson (III:1), John Erskine (IV:4), Clark Leonard Hull (IV:1), James Robertson Jack (I:4), Dugald Caleb Jackson (I:4), Lewis Jerome Johnson (I:4), Carl Otto Lampland (I:1), Morris Evans Leeds (III:4), Frederic Thomas Lewis (II:3), Ralph

---

[7] Including linguistics, literary and art scholarship, etc.

Stayner Lillie (II:3), Roswell Cheney McCrea (III:3), Edward Leyburn Moreland (I:4), William de Berniere MacNider (II:4), James Lee Peters (II:3), Anthony John Philpott (IV:4), Charles Lane Poor (I:1), Martin André Rosanoff (I:3), John Sloan (IV:4), Donald Charles Stockbarger (I:2), Thomas Wayland Vaughan (II:1), Karl Viëtor (IV:4), Edmund Allen Whitman (FE III:1), John Zeleny (I:2); Six Foreign Honorary Members—René Maunier (III:3), Kingo Miyabe (II:2), Serge Koussevitzky (IV:4), Hanns Oertel (IV:3), Charles Scott Sherrington (II:4), Nevil Vincent Sidgwick (I:3).

Four Fellows have been classed as Emeriti: Giuseppe Antonio Borgese (IV:3), William Carter Quinby (II:4), George Sarton (IV:2), Martin Wagner (IV:4).

Four Fellows have resigned: Luther Harris Evans (IV:4), Ralph Linton (IV:2), Isidor Isaac Rabi (I:2), Thomas Walter Swan (III:1).

In May, 1951, fifty-eight Fellows and five Foreign Honorary Members were elected to the Academy; all except seven accepted election.

The roll now includes 911 Fellows, 58 Fellows Emeriti, and 142 Foreign Honorary Members (exclusive of those to be elected tonight).

Respectfully submitted,

May 14, 1952 HENRY B. PHILLIPS, *Secretary*

Report of the Treasurer

The Treasurer presented his usual full report in print, which Fellows may obtain on application to him. The following tables give Receipts and Expenditures for the fiscal year ending March 31, 1952.

RECEIPTS

Academy, General:		
Investment Income	$22,840.79	
House Charges	4,257.85	
Assessments and Admission Fees	8,506.00	$35,604.64
Mabel S. Agassiz Fund:		
Investment Income		124.00
Amory Fund Committee:		
Investment Income		4,104.40
Linda Hall Library Publication Exchange Fund		2,025.00
Kennelly Fund:		
Investment Income		111.60
Life Membership Fund—T. H. Brown		200.00

Permanent Science Fund:		
Boston Safe Deposit and Trust Co., Trustee	$15,000.00	
Return of Grants	1,504.58	
Investment Income	744.00	17,248.58
Publications Committee:		
Investment Income	$ 3,385.20	
Sale of Publications	1,170.53	
Lake Publications	199.01	4,754.74
Rockefeller Foundation Fund:		
Institute for the Unity of Science		3,000.00
Rumford Fund Committee:		
Investment Income	$ 5,217.30	
Return of Grants	310.00	
Sale of Publications	40.00	5,567.30
Special Endowment Fund:		
Investment Income		12.40
Warren Fund Committee:		
Investment Income		1,221.40
Total		$73,974.06

EXPENDITURES

Academy, General:		
Salaries and Pensions, etc.	$13,205.24	
Meetings and General Administration	7,012.60	
House Maintenance	9,643.70	
President's Expense	181.82	
Treasurer's Expense	709.15	
Special Appropriations:		
School Science Fund	598.37	
Committee on International Relations	270.26	$31,621.14
Transfer to Current Expense Fund		3,983.50
		$35,604.64
Mabel S. Agassiz Fund:		
For Meetings		124.00
Amory Fund:		
Expense		85.11
Institute for the Unity of Science		4,654.70

Permanent Science Fund:		
Grants	$24,315.00	
Expense	341.54	24,656.54
Publications Committee:		
Bulletin	$ 1,382.10	
Proceedings	6,475.63	
Miscellaneous	17.74	7,875.47
Rumford Fund Committee:		
Award	$ 500.00	
Medal	750.60	
Expense	2,673.87	3,924.47
Total		$76,924.93

Respectfully submitted,

May 14, 1952

Horace S. Ford, *Treasurer*

Report of the House Committee

The Academy used the House forty-seven times for its own meetings during the past year, with an average attendance of forty-one per meeting. Besides the eight stated meetings of the Academy, there were thirty-nine meetings of various committees and conferences under the auspices of the Academy. A total of thirty-two other scientific or cultural guest organizations held 165 meetings in the House, with an average attendance of seventy-six. It is estimated that the total attendance at meetings and exhibits for the year was about 15,900. The table below lists the various guest organizations and the number of meetings held by each.

	No. of Meetings
Alliance Française (one meeting with Salon Français)	7
Alpha Omega Fraternity, Tufts Dental School	5
Alpha Phi Sorority (Graduates of Salem Teachers College)	1
Backus Historical Society	1
Boston Authors Club	12
Boston Psychoanalytic Society and Institute	9
Boston Shakespeare Club	1
Boston Society of Civil Engineers	5
Boston Teachers Union	12
Brookline Bird Club	2
Children's Museum Trustees	3
China Students' Club	8

Colonial Dames	2
Dynamion Society	17
French Center of New England	8
French Consulate	2
French Library in Boston	1
Green Mountain College Alumni	1
High School Women's Club of Boston	2
Institute of Contemporary Art	8
Joyce Kilmer Post, American Legion and Auxiliary, etc.	25
Massachusetts General Hospital Symposium	2
Mediaeval Academy	2
New England Botanical Club	9
New England Section, Optical Society of America	6
New England Woman's Press Association	6
Pan American Society of New England	1
Polish Literature Club	1
Rhodes Scholarship Committee (Mass. and New England)	2
Salon Français	1
Society for Preservation of Antiques	2
United Prison Association of Massachusetts	1
	165

The next table below gives the titles of the exhibits held in the lobby and the period during which they were displayed. It is estimated that about 1,500 entered the House solely to view the exhibits.

Twenty-seven Presidents of the Academy, portraits. October.

Lost Architectural Masterpieces Regained, Reconstruction Studies, by Kenneth Conant. October.

One Hundred Color Transparencies of Masterpieces from the Louvre. November 5 to 30.

Benjamin Franklin and the Science of His Time. December and January.

Exhibit of Activities of the Academy and of the Committee on International Scientific Publication. February and March.

Prisons. April.

Synthetic Elements. April and May.

The Lowell Institute Cooperative Broadcasting Council, whose office had been in the large room (1,164 sq. ft.) on the fourth floor of the building since the beginning of their organization in 1947, moved to larger quarters in Symphony Hall in October, 1951. This space still remains available for use. The Committee on International Scientific Publication continued to use the smaller office (570 sq. ft.) on the fourth floor, and the Academy offices continued on the first floor. The Boston Museum of Science and the Boston Authors Club continued to share space with us in the book-stack wing.

PROCEEDINGS

OF THE

AMERICAN ACADEMY

OF

ARTS AND SCIENCES

VOL. LXXXI

INCLUDING RECORDS
FROM OCTOBER 1951 TO MAY 1952

BOSTON:
PUBLISHED BY THE ACADEMY
1951 — 1952

CONTENTS

Issue | Page

1. *Adventures with Standing Light Waves (Rumford Medal Lecture, 1951).* Herbert E. Ives 1

2. *Biographical Digests IV: Centennials and Polycentennials during 1952 with Interest for Chemists and Physicists.* Ernest H. Huntress 33

3. *Officers and Committees for 1951-1952* 102
List of the Fellows and Foreign Honorary Members by Class and Section 105
Alphabetical Index to Lists of Members 134
Statutes 147
Charter of Incorporation 162

4. *The Resistance of 72 Elements, Alloys, and Compounds to 100,000 kg/cm^2.* P. W. Bridgman. 165

5. *Report on the White Dwarf Survey of the Southern Hemisphere.* Willem J. Luyten. 253

6. *The Problem of Underdeveloped Areas in Asia and Africa.* Francis B. Sayre 283

7. *Records of Meetings, October, 1951 to May, 1952* 299

The costs of maintaining and operating the House are given in the table below:

Expenditures	*1950-1951*	*1951-1952*
Custodian, Assistance, Pensions	$ 3,548.75	$ 3,753.93
Coal	1,343.01	1,202.60
Gas	23.13	3.10
Electricity	1,385.20	1,377.21
Elevator	933.83	1,489.50
Water	25.50	25.50
Equipment	750.45	618.42
Supplies	591.01	428.30
Alterations and Repairs	1,582.78	757.25
Fire Insurance	594.62	520.05
Miscellaneous	55.00	67.84
Total	$10,833.28	$10,243.70
Appropriations (including pension)	12,085.00	11,875.00
Unexpended	$ 1,251.72	$ 1,631.30

Receipts for use of the facilities of the House by other scientific, educational, and cultural organizations reduced the net cost to the Academy as follows:

	1950-1951	*1951-1952*
Total operating expenditures	$10,833.28	$10,243.70
Receipts for the year	4,100.56	4,257.85
Net cost of House Maintenance and Operation to the Academy	$ 6,732.72	$ 5,985.85

During the year we purchased a Bell and Howell, 16 mm. sound, motion picture projector and an American Optical, Delineascope, for projecting 2 x 2 slides and 35 mm. film strips. These are made available, along with our standard slide projector, to guest organizations using the Lecture Hall. Spring door closers and new interlocks were installed on each of the five floors of the elevator well, as well as closer for the gate of the elevator car. On July 1 we instituted a maintenance contract with the Otis Elevator Company. The fire escape on the rear of the House was painted.

Respectfully submitted,

May 14, 1952

CHESTER M. ALTER, *Chairman*

Report of the Committee on Publication

The Committee held one formal meeting during the year. The rest of the business was conducted by correspondence and telephone.

No further numbers of the *Memoirs* were published during the year. Volume 79 of the *Proceedings* was completed with the Summer Number in August, 1951. It contains 356 pages as compared with 272 pages of Volume 78.

In accordance with the agreement reached last year with the Institute for the Unity of Science (*Proceedings*, Volume 79, page 345), Volume 80 started publication "in co-operation with the Institute for the Unity of Science." Number 1, dated July, but distributed in the fall, was sent to all Fellows. The second number is in page proof and should be distributed in June. One or two more numbers will follow in 1952, bringing the total number of pages to 400. Volume 81 of the *Proceedings* is running concurrently with this. Six numbers have appeared and the volume will be completed with the Summer Number. This volume contains two papers that were presented at meetings of the Academy: The Rumford Medal Lecture by Herbert E. Ives and the communication of Francis B. Sayre, presented before the Academy at the April meeting. These papers are, therefore, really "Proceedings" in the sense in which the term was originally understood.

The *Bulletin*, under the direction of Mr. Burhoe, has been issued eight times, and, as usual, carried notices and abstracts of the communications presented at the meetings, except for the two which were printed in their entirety in the *Proceedings*.

The Committee on Publication received for the year $1,369.54 from sales of publications, and $3,385.20 from investment income, a total of $4,754.74. It received in addition from the Rumford Committee $2,653.30 to pay the cost of printing *The Sun in the Service of Man*. Expenses for the year, not counting the number of the *Proceedings* paid for by the Rumford Committee, were $7,875.47. The total expenses were actually $10,528.77, and the total receipts $7,408.04, showing an excess of outlay totaling $3,120.73. This deficit was charged against the credit balance which has shrunk further from $11,809.76 on March 31, 1951 to $8,689.03. Of this balance, $1,409.04 belongs to the Lake Fund. At the present rate this reserve will be exhausted by March 31, 1954.

There is now in type the first number of a series of Slavic Studies, edited by two Fellows of the Academy, Roman Jakobson and Michael Karpovich. This series, the costs of which will be underwritten by a special fund to support Slavic Studies, the Reisinger Fund, will appear as a special series at the rate of two or three numbers a year. The October number of the *Bulletin* will carry a full announcement.

The publication policy of the Committee is at present governed by the vote of April 11, 1951, which was printed in last year's report. At that time the Editor requested suggestions and comments on this program. None has been received. The policy of the Committee is naturally controlled by the present high costs of printing. No special publications can be financed on the present regular income from sales and investment. Consequently, the eleventh volume of the *Dated Greek Manuscripts*, which is ready, cannot be produced until the balance in the Lake Fund, $1,409.04, is augmented by gifts or special appropriations.

Respectfully submitted,

May 14, 1952
TAYLOR STARCK, *Chairman*

Report of the Permanent Science Fund Committee

At our October meeting we had 15 applications to consider, totaling requests for $18,330, of which 9 were acted upon favorably involving grant awards of $9,065. Six of the applications were declined.

In March, 13 applications requesting a total of $13,042 were presented for consideration. The 1951-1952 income of the Fund was $16,504 but, with our reserve, $24,399 was available as of March 1, 1952. Of the 13 applications, 3 were declined and 10 were approved, for a total amount of $8,405.

Reports of the specific grants to individuals were made to the Council of the Academy on October 10, 1951 and on March 10, 1952, as follows:

Grants Approved October 10, 1951

1. To Mark D. Altschule, M.D., Assistant Professor of Medicine, Harvard Medical School; Director of Internal Medicine and of Research in Clinical Physiology, McLean Hospital, Waverley 79, Massachusetts, for an investigation of intermediary carbohydrate metabolism in mental disorders, $1,500.

2. To Henry N. Andrews, Jr., Professor of Botany, Washington University Botany School, St. Louis 5, Missouri, for studies of the petrified carboniferous plants of the central American coal fields, $500.

3. To Benjamin Karpman, M.D., Chief Psychotherapist, Saint Elizabeths Hospital, Washington, D. C., for studies in the psychogenetics of crime as a basis for more general study of psychodynamics of human behavior, $1,000.

4. To Maynard Mosely, Assistant Professor of Botany, Santa Barbara College, University of California, Santa Barbara, California, for a study of the comparative anatomy and phylogeny of the Garryaceae, $545.

5. To Dr. Fritz Reder, Assistant, Physics Dept., University of Graz, Austria, for detection of gas impurities by microwave gas breakdown measurements, $820.

6. To George H. Scherr, Assistant Professor, The Creighton University School of Medicine, Department of Microbiology, Omaha, Nebraska, for a study of the effect of gonadotropic hormones on systemic histoplasmosis in mice, $1,000.

7. To Oswald H. Robertson, Lecturer in Biology, Department of Biological Sciences, Stanford University, California, for a study of the cause of death of the Pacific Salmon following spawning, $1,200.

8. To Charmion Shelby, Assistant Editor, *Handbook of Latin American Studies*, Hispanic Foundation, Library of Congress, Washington, D. C., for the editing of her English translation of Garcilaso de la Vega's *La Florida del Inca*, for publication by the Hakluyt Society in 1953 or 1954, $1,000.

9. To Hans H. Zinsser, M.D., Assistant Clinical Professor, Dept. of Surgery, U. S. C. Medical School, Los Angeles, California, for the isolation and inhibition of bacterial urease from organisms of urological importance, $1,500.

Grants Approved March 10, 1952

1. To Edgar Altenburg, Associate Professor of Biology, The Rice Institute, Houston, Texas, for an investigation of the influence of ultraviolet light on mutagenesis in *Drosophila*, $1,500.

2. To Robert H. Cole, Professor of Chemistry, Metcalf Research Laboratory, Brown University, Providence, Rhode Island, for further work on high field conductances of paraffin chain electrolytes in water and other polar solvents, $850.

3. To A. Hunter Dupree, Assistant Professor of History, Texas Technological College, Lubbock, Texas, for a biography of Asa Gray, $1,000.

4. To Rose Frisch, Laboratory Assistant, Harvard University, Cambridge, Massachusetts, for studies of the transformation of mutant *Drosophila* larvae to wild type phenotype by their feeding on wild type pupae, $750.

5. To Reginald Ruggles Gates, Emeritus Professor, University of London, Harvard University, Cambridge, Massachusetts, for studies of skin color inheritance, $155.

6. To C. Clayton Hoff, Associate Professor of Biology, University of New Mexico, Albuquerque, New Mexico, for studies on the pseudoscorpions of New Mexico, $400.

7. To Hallam L. Movius, Jr., Associate Professor, Harvard University, Cambridge, Massachusetts, for a study of the origin, development, chronology, and affinities of the Palaeolithic cultures of the territories of the U. S. S. R., $750.

8. To George R. Rumney, Assistant Professor of Geography, University of Connecticut, Storrs, Connecticut, for a determination, from the latest statistical data, of trends and probable outcome of settlement expansion in the relatively inhospitable region of the Canadian Shield, $1,000.

9. To Harry Sobotka, Chemist to The Mount Sinai Hospital, New York 29, New York, for an investigation of the monomolecular film techniques for microanalyses, especially of keto-steroids, $1,500.

10. To Robert E. Woodson, Jr., Curator of Herbarium, Missouri Botanical Garden, St. Louis 10, Missouri, for a biometrical analysis of natural selection in *Asclepias tuberosa* L. (Butterflyweed), $500.

Respectfully submitted,

May 14, 1952

HUDSON HOAGLAND, *Chairman*

Report of the Rumford Committee

In early September, 1951, the Academy published the 146-page issue of its *Proceedings* (Vol. 79, No. 4) containing the papers presented at the Conference on the Sun in the Service of Man, arranged and supported by the Rumford Committee. To cover the cost of the publication, $2,653.30 of the

Rumford Fund was appropriated. On May 9 and 10, 1952, the Academy held at its House a "Symposium on Climatic Change," supported by $1,000 from the Rumford Fund. About 80 invited conferees participated in the discussion of some 20 papers collating evidence from several sciences concerning the facts and explanatory hypotheses of climatic change in the history of the planet. These papers are to be published by the Academy.

On November 14, 1951, the 1951 Rumford Medal was presented to Dr. Herbert E. Ives, of Montclair, New Jersey, for his noteworthy contributions to optics. His acceptance address to the Academy was published as a special issue of the *Proceedings,* Vol. 81, No. 1, with the expectation that it would inaugurate a continuing series of biennial Rumford Medal Lectures.

In October, 1951, a grant of $500 was made from the Rumford Fund to Professor Forrest F. Cleveland, Department of Physics, Illinois Institute of Technology, Chicago, in support of his researches on Raman and infrared spectral data, assignments, force constants, and thermodynamic properties of substituted methanes and ethanes.

Plans for the celebration of the bicentennial of the birth of Rumford were developed. A committee on the celebration was established and $1,000 from the Fund was appropriated in April, 1952, for the preparation under the direction of Professor Sanborn Brown of an exhibit of models of Rumford's philosophical apparatus.

The Treasurer reports that the unexpended income of the Rumford Fund, as of March 31, 1952, was $27,130.94.

Respectfully submitted,

May 14, 1952 HARLOW SHAPLEY, *Chairman*

Report of the Cyrus M. Warren Committee

The Committee had available for grants the sum of fifteen hundred dollars, which amount had been voted by the Council at the meeting on March 14, 1951. The Committee met on May 8, 1951, for consideration of the applications that had been received. Due to an inaccurately printed notification in *Science* regarding the Fund, all applications had been presented for amounts much larger than the Fund permitted. Therefore, after due consideration by the Committee, it was voted not to use the appropriation for 1951-52, but to hold the amount for use together with the appropriation for 1952-53.

A report has been received from Professor John D. Spikes, University of Utah, indicating the disposition of a grant of $150 in 1950. Professor Spikes reports that the equipment obtained is in constant use, and further reports will be made when published results are available.

Reports of progress in his research have been received from Professor A. J. Castro of San José State College, who received a grant of $55 in 1950.

Reprints have been received as follows:

From G. R. Seaman: "Enzyme Systems in Tetrahymena Geleii S. II. Acetylcholinesterase Activity. Its Relation to Motility of the Organism and

to Coordinated Ciliary Action in General." by G. R. Seaman and R. K. Houlihan. *Jour. Cellular and Comparative Physiology, 37*, 309 (1951).
From E. C. Gilbert: " Heats of Combustion of Some Isomeric Nitrostilbenes." by C. M. Anderson, L. G. Cole, and E. C. Gilbert. *J. Am. Chem. Soc.*, *72*, 1263 (1950).

Respectfully submitted,
May 14, 1952 FREDERICK G. KEYES, *Chairman*

Report of the Amory Prize Committee

The Committee on the Amory Prize met on November 21, 1951, with all 7 members and the Executive Officer present. It was noted that the Kretschmeyer Foundation of Chicago had recently been set up to operate in the same field as that of the Amory Committee, and it was resolved to keep in touch with that foundation. Further names for consideration in connection with the next award were added to the list. The policy of dividing the award between a number of persons (4 for the septennium ending November 10, 1940, and 6 for that ending in 1947) was discussed, as was the policy of repeating awards to earlier recipients who had continued to obtain notable results. One of the most serious questions before the Committee as the time for new awards approaches will be how to obtain the best nominations; previous efforts to attract well-considered nominations from outside the Committee have not been very successful.

Respectfully submitted,
May 14, 1952 EDWIN B. WILSON, *Chairman*

OFFICERS AND COMMITTEES ELECTED MAY 14, 1952

For a complete list of officers and committees see the first pages of this issue.

President	Edwin H. Land
Vice-President for Class I	Jerome C. Hunsaker
Vice-President for Class II	Alfred S. Romer
Vice-President for Class III	Erwin D. Canham
Vice-President for Class IV	Wallace Goodrich
Secretary	Henry B. Phillips
Treasurer	Horace S. Ford
Librarian	Ernest H. Huntress
Editor	Taylor Starck

Councillors

To serve for four years:

John T. Edsall, *of Class I*
Francis M. Rackemann, *of Class II*
Chester M. Alter, *of Class III*
Hugh Hencken, *of Class IV*

House Committee

John B. Wilbur .... *to serve for one year,* and as *Chairman*
Philippe E. LeCorbeiller .... *to serve for two years*
Henry R. Viets .... *to serve for three years*

Committee on Membership

To serve for two years:

Edward M. Purcell, *of Class I*
James M. Faulkner, *of Class II*
Walter M. Whitehill, *of Class III*
Mason Hammond, *of Class IV*

Committee on Meetings

To serve for two years:

Bart J. Bok, *of Class I*
Roy G. Hoskins, *of Class II*

Committee on Finance

To serve for two years:

Thomas D. Cabot
Ralph Lowell

Auditing Committee

To serve for two years:

Bishop C. Hunt

Committee on Publication

To serve for two years:

Philipp G. Frank, *of Class I*
Frank M. Carpenter, *of Class II*

Permanent Science Fund Committee

To serve for three years:

John L. Oncley
Reed C. Rollins
Chester S. Keefer

Rumford Committee

George R. Harrison .... *to serve for one year,* and as *Chairman*
Norman F. Ramsey .... *to serve for three years*
George Wald .... *to serve for three years*

C. M. Warren Committee

To serve for three years:

Edwin R. Gilliland, *Chairman*
Robert H. Cole

Amory Prize Committee

To serve for one year:

Edwin B. Wilson, *Chairman*

William B. Castle
Roy G. Hoskins
Gregory Pincus
George W. Thorn
George B. Wislocki
S. Burt Wolbach

Nominating Committee

To serve for three years:

Harlow Shapley, *of Class I, Chairman*
Clifford Frondel, *of Class II*
Robert B. Stewart, *of Class III*
William F. Twaddell, *of Class IV*

NEW MEMBERS ELECTED MAY 14, 1952

FELLOWS (84)

Class I—Mathematical and Physical Sciences (22)

Section 1—Mathematics and Astronomy (1)

Raphael Salem	Mass. Institute of Technology, Cambridge, Mass.

Section 2—Physics (5)

Bruce Hadley Billings	Baird Associates, Cambridge, Mass.
Richard Henry Bolt	Mass. Institute of Technology, Cambridge, Mass.
Henry Margenau	Yale University, New Haven, Conn.
Wayne Buckles Nottingham	Mass. Institute of Technology, Cambridge, Mass.
Robert Vivian Pound	Harvard University, Cambridge, Mass.

Section 3—Chemistry (9)

Halvor Niels Christensen	Tufts Medical School, Boston, Mass.
Lawrence Joseph Heidt	Mass. Institute of Technology, Cambridge, Mass.
Donald Frederick Hornig	Brown University, Providence, R. I.
John Withers Irvine, Jr.	Mass. Institute of Technology, Cambridge, Mass.
Gilbert William King	Arthur D. Little, Inc., Cambridge, Mass.
John Dombrowski Roberts	Mass. Institute of Technology, Cambridge, Mass.
Gerhard Schmidt	Tufts Medical School, Boston, Mass.
Arnold Max Seligman	Harvard Medical School, Boston, Mass.
Cutler D. West	Polaroid Corporation, Cambridge, Mass.

Section 4—Technology and Engineering (7)

Manson Benedict,	Mass. Institute of Technology, Cambridge, Mass.
Leo LeRoy Beranek	Mass. Institute of Technology, Cambridge, Mass.
John Anthony Hrones	Mass. Institute of Technology, Cambridge, Mass.
Harold Somers Mickley	Mass. Institute of Technology, Cambridge, Mass.
John Alvin Pierce	Harvard University, Cambridge, Mass.
Ascher Herman Shapiro	Mass. Institute of Technology, Cambridge, Mass.
Herbert Henry Uhlig	Mass. Institute of Technology, Cambridge, Mass.

Class II—Natural and Physiological Sciences (21)

Section 1—Geology, Mineralogy, and Physics of the Globe (4)

Henry Garrett Houghton	Mass. Institute of Technology, Cambridge, Mass.
Morris Morgan Leighton	Illinois Geological Survey, Urbana, Illinois
Raymond Cecil Moore	University of Kansas, Lawrence, Kansas
Hurd Curtis Willett	Mass. Institute of Technology, Cambridge, Mass.

Section 2—Botany (4)

Ralph Emerson	University of California, Berkeley, Calif.
Richard Hale Goodwin	Connecticut College, New London, Conn.
James Gordon Horsfall	Connecticut Agricultural Station, New Haven, Conn.
G(eorge) Ledyard Stebbins, Jr.	Experimental Station, Davis, Calif.

Section 3—Zoology and Physiology (9)

Donald Leslie Augustine	Harvard University, Cambridge, Mass.
Edgar John Boell	Yale University, New Haven, Conn.
Leigh Edward Chadwick	Army Chemical Center, Baltimore, Maryland
George Leonard Clarke	Harvard University, Cambridge, Mass.
Donald Redfield Griffin	Cornell University, Ithaca, N. Y.
George Wallace Kidder	Amherst College, Amherst, Mass.
Earl Randall Loew	Boston University School of Medicine, Boston, Mass.
Irwin Whiting Sizer	Mass. Institute of Technology, Cambridge, Mass.
George Davis Snell	Jackson Memorial Lab., Bar Harbor, Maine

Section 4—Medicine and Surgery (4)

Francis Daniels Moore	Harvard Medical School, Boston, Mass.
Harry Caesar Solomon	Harvard Medical School, Boston, Mass.
Frederick John Stare	Harvard School of Public Health, Boston, Mass.
Carl Tenbroeck	Jackson Memorial Lab., Bar Harbor, Maine

CLASS III—THE SOCIAL ARTS (22)

Section 1—Jurisprudence (1)

William Henry Hastie	U. S. Court of Appeals, Third Circuit, Philadelphia, Pa.

Section 2—Government, International Law and Diplomacy (7)

Charles Richard Cherington	Harvard University, Cambridge, Mass.
Robert Eugene Cushman	Cornell University, Ithaca, N. Y.
Thomas Hopkinson Eliot	Boston, Mass.
George Frost Kennan	Ambassador to the U. S. S. R., Moscow, Russia
Norman Judson Padelford	Mass. Institute of Technology, Cambridge, Mass.
Christopher Hallowell Phillips	Massachusetts Senator, North Beverly, Mass.
Margaret Chase Smith	U. S. Senator from Maine, Skowhegan, Maine

Section 3—Economics and Sociology (1)

John Kenneth Galbraith	Harvard University, Cambridge, Mass.

Section 4—Administration and Affairs (13)

Walter Scott Baird	Baird Associates, Cambridge, Mass.
Alan Barth	Washington Post, Washington, D. C.
John Otis Brew	Harvard University, Cambridge, Mass.

Harold Claude Case	Boston University, Boston, Mass.
Margaret Clapp	Wellesley College, Wellesley, Mass.
John Henshaw Crider	Lincoln, Mass.
David Dubinsky	International Ladies Garment Workers Union, New York, N. Y.
Frank Porter Graham	U. N. Representative to Kashmir
Jacob George Harrar	Rockefeller Foundation, New York, N. Y.
Paul Gray Hoffman	Ford Foundation, Pasadena, Calif.
Harrison Keller	New England Conservatory of Music, Boston, Mass.
Walter Philip Reuther	International Union, United Automobile, Aircraft, and Agricultural Workers of America, C. I. O., Detroit, Mich.
Earl Place Stevenson	Arthur D. Little, Inc., Cambridge, Mass.

Class IV—The Humanities (19)

Section 1—Theology, Philosophy, and Psychology (4)

Irwin Edman	Columbia University, New York, N. Y.
Charles William Morris	Chicago University, Chicago, Ill.
Burrhus Frederic Skinner	Harvard University, Cambridge, Mass.
Hao Wang	Harvard University, Cambridge, Mass.

Section 2—History, Archaeology, and Anthropology (5)

I. Bernard Cohen	Harvard University, Cambridge, Mass.
Hallam Leonard Movius, Jr.	Harvard University, Cambridge, Mass.
France Vinton Scholes	University of New Mexico, Albuquerque, N. M.
Clifford Kenyon Shipton	American Antiquarian Society, Worcester, Mass.
Gordon Randolph Willey	Harvard University, Cambridge, Mass.

Section 3—Philology (4)

Peter Boodberg	California University, Berkeley, Calif.
John Peterson Elder	Harvard University, Cambridge, Mass.
George Sherman Lane	Chapel Hill, N. C.
Wolf Leslau	Brandeis University, Waltham, Mass.

Section 4—The Fine Arts and Belles Lettres (6)

Alfred Hamilton Barr, Jr.	Museum of Modern Art, New York, N. Y.
Pietro Belluschi	Mass. Institute of Technology, Cambridge, Mass.
Gyorgy Kepes	Mass. Institute of Technology, Cambridge, Mass.
Meyer Schapiro	Columbia University, New York, N. Y.
Lionel Trilling	Columbia University, New York, N. Y.
Elwyn Brooks White	New York, N. Y.

FOREIGN HONORARY MEMBERS (7)

CLASS I

Section 2—Physics (2)

Cornelius Gorter	Kamerlingh Onnes Laboratory, Leiden, Netherlands
Franz Eugen Simon	Oxford University, Oxford, England

CLASS II

Section 3—Zoology and Physiology (1)

Karl von Frisch	Graz University, Graz, Austria

Section 4—Medicine and Surgery (1)

James Bertram Collip	University of Western Ontario, London, Ontario, Canada

CLASS III

Section 4—Administration and Affairs (2)

Geoffrey Crowther	The Economist, London, England
Jaime Torres Bodet	UNESCO, Paris, France

CLASS IV

Section 2—History, Archaeology, and Anthropology (1)

Alfonso Caso Andrade	National Institute of Anthropology and History, Mexico, D. F., Mexico

INDEX TO VOLUME 81

A. INDEX OF PAPERS BY AUTHOR AND BY TITLE

Adventures with Standing Light Waves (Rumford Medal Lecture, 1951), 7

Biographical Digests IV: Centennials and Polycentennials during 1952 with Interest for Chemists and Physicists, 35

Bridgman, P. W., *The Resistance of 72 Elements, Alloys, and Compounds to 100,000 kg/cm*^{2}, 169

Huntress, Ernest H., *Biographical Digests IV: Centennials and Polycentennials during 1952 with Interest for Chemists and Physicists*, 35

Ives, Herbert E., *Adventures with Standing Light Waves*, 7

Luyten, Willem J., *Report on the White Dwarf Survey of the Southern Hemisphere*, 255

Problem of Underdeveloped Areas in Asia and Africa, 284

Report on the White Dwarf Survey of the Southern Hemisphere, 255

Resistance of 72 Elements, Alloys, and Compounds to 100,000 kg/cm^2, 169

Sayre, Francis B., *The Problem of Underdeveloped Areas in Asia and Africa*, 284

B. INDEX TO AFFAIRS OF THE AMERICAN ACADEMY OF ARTS AND SCIENCES

Alphabetical index to Lists of Members, 134

Annual meeting, 309

Appropriations, 307

Assessment, annual, 307

Award for Outstanding Science Teaching, 308

Charter of Incorporation, 162

Classification of members, 309

Committees, lists of and members of: 1951–1952, 102; 1952–1953, 301
- Amory Prize, 103, 160, 303, 321
  - Report of, 320
- Auditing, 103, 158, 302, 321
- Classification of Membership, 104
- Conference on Climatic Variations, 104, 303
- Finance, 103, 158, 302, 321
  - Report of, *see* Treasurer, 311
- House, 102, 158, 301, 321
  - Report of, 313
- International Relations, 104, 303
- Meetings, 102, 158, 302, 321
- Membership, 102, 158, 301, 321
- Nominating, 103, 160, 303, 321
- Permanent Science Fund, 103, 159, 302, 321
  - Grants, 317
  - Report of, 317
- Planning and Development, 104, 303
- Policy on Membership and Dues, 304
- Publication, 103, 158, 302, 321
  - Report of, 316

Committees, continued
- Rumford, 103, 159, 302, 321
  - Award of 1951 Premium, 305
  - Rumford Medal Lecture, 1951, 7, 305
  - Report of, 318
- School Science, 104, 304, 308
- Science and Values, 104, 304
- Solar Energy, 104
- Statutory Amendments, 104
- Unity of Science, 104, 304
- Warren, C. M., 103, 159, 303, 321
  - Report of, 319

Council, 102, 157, 301, 320
- Statutory amendment to increase number of Councillors to 20, 310

Elections
- Fellows and Foreign Honorary Members, 322
- Officers and Committees, 320

Exhibits, 314

Fellows, list of, 105
- Deaths, 310
- Elected, 322
- Introductions (Indicated in Records of Meetings), 305
- Resignations, 311
- Transfer of Status, 311

Foreign Honorary Members, list of, 129
- Deaths, 311
- Elected, 324

Funds, *see under* the respective committees in charge

Introductions, *see* Fellows, Introductions

List of the Fellows and Foreign Honorary Members, 105

Meetings, records of, 305

Members, alphabetical index, 134
  Classification of, 309
  see also: Fellows, Foreign Honorary Members

Officers and Committees: 1951–1952, 102; 1952–1953, 301

Publications, *see* Committee on Publication

Records of meetings, 305

Reports, *see* under respective officers, committees, etc.

Rumford Committee, *see* Committees, Rumford
Rumford Medal Lecture, 3, 7, 305

Rumford Premium, 305
  Recipients, 5

Statutes, 147
  Amendment to, 309

Treasurer, report of, 311